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AUTOMATIC WRITING

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I. Introduction

The purpose of the investigation under consideration was, if possible, to obtain samples of automatic writing in order to determine (1) Under what conditions such writing appears, and (2) What changes take place in the appearance of writing as it becomes less and less controlled.

The first problem involved a study of the mental make-up of a given reagent relative to the scope of attention, tendency to automatism, nature of the inner speech, etc.

The second problem would include a comparison of the writing produced under distraction with the normal writing of a given reagent with reference to many different writing-factors such as size, alignment, slant, line-quality, form and the like. Such a comparison is highly desirable in connection with the study of handwriting for diagnostic purposes since it is of prime importance to know what changes appear as writing becomes progressively uncontrolled. Such a knowledge would also be of value in investigating the alleged shifts of handwriting individuality in mediumistic writing.

In order to obtain material for discussion of the above-stated problems the following experiments were tried:

I. Preliminary experiments, reagent took dictation and wrote sentences or answered questions while reading silently.

II. Reagent wrote a given verse, perfectly memorized, continuously, during the silent reading of the chapters of a novel.

III. Reagent wrote a given verse, perfectly memorized, during the continuous adding of columns of figures.

IV. The reagent wrote, continuously, a given verse, perfectly memorized, while reading aloud.

V. The reagent wrote from dictation (1) while reading aloud, (2) while reading silently.

In reporting our results we confine ourselves to their bearing upon the first problem stated.

Before a detailed account is attempted of the conditions holding in the different series of experiments, a general statement of the conditions that were constant throughout the whole may be given. The experiment ran continuously for seven months (with the breaks incident to college organization), occupying an afternoon session from one to two hours long for five days in the week. The two writers of the paper (A and D) served in turn as reagents.

The reagent in every case sat to the left of a black screen, in the lower half of which was a loose sleeve for the arm opening toward the right in such a fashion that when the hand and arm had been run through it, they were perfectly concealed from the reagent and rested comfortably and without constraint upon the writing-pad which lay on the table, to the end of which the screen was clamped. The experimenter sat to the right of the screen by the table. He was, therefore, out of the reagent's sight. His part in the experiment consisted in manipulating the writing-pad, timing by means of a stop-watch the writing of the reagent, and taking down at set intervals the introspections of the reagent. The writing was done, in part, in pencil, in part, in ink.

A few words as to the reagents. Neither shows any striking automatic tendencies, neither is absent-minded, both are averse to "letting themselves go" and maintain control to a high degree. Both show certain perseverative tendencies, are apt to be obsessed by their work, have difficulty in getting to sleep, etc.

Whatever automatisms appear for D would seem to be sensory in character. D has little control over her imaginal processes and is often greatly surprised by the course of her images, she is strikingly "intuitive" in certain lines of work. With this there goes a consciousness of movement to an extraordinary extent so that D is able to inhibit motor expression to an unusual degree. She is a "bad subject" for the muscle-reader. Such inhibition of effort and excess of control are evident in her handwriting which is small, labored, and self-conscious. Sudden impulses leading to excessive

movement occur, however, at intervals and under certain physical conditions D's writing becomes free, fluid, and large.

A is very easy and rapid in his movements, his writing is unusually rapid and smooth in impulse, although slightly illegible. He cuts strokes and ignores the visual appearance of his letters. Attention is focused on meaning, leaving graphic details to habit. A is an excellent subject for the muscle-reader. He reports at times, however, muscular cramp.

While A's writing movements are much freer and smoother than D's, there is some evidence of greater vocal-motor ease for D; thus D's speed in reading aloud is slightly higher than A's, although A was much the more rapid in silent reading. D was able to vocalize mentally more rapidly than could A.

Both reagents show considerable scope of attention[1] and considerable rapidity in work. A's rapidity in executing and synchronizing two processes would seem to be quite exceptional. A great number of tests have shown that he is capable of sustained and effective attention. D's attention is more apt to wander, she was less successful than A in maintaining the double processes at high speed; to some extent this result may be due to her failure to turn over motor details to habit. She maintains a constant supervision over them with this consequence that she makes very few word or letter lapses in comparison with A but when she does make a lapse, it is much more significant of automatism than are A's.

II. Preliminary Experiments

These experiments were tried mostly in the spring of 1913 that we might get some idea of the task in hand and some preliminary practise. At first the reagent simply made loop- or roof-strokes continuously while reading silently. After a few trials both of us were able to do this without any effort. Then the reagent tried taking dictation while reading to himself. The reagent began reading and continued making loops until the experimenter dictated a sentence, a word at a time. He wrote this sentence repeatedly until the experimenter dictated another. The record of reagent A is given to illustrate what happened.

A did not find this task at all difficult introspectively. An

[1] In part, we judge this from their reading-rate. Dearborn writes, "A wider 'spanning' of attention,—as denoted both by the greater frequency of long pauses at the beginning of the line and by fewer fixations per line,—is characteristic of the more rapid readers." The Psychology of Reading, *Arch. of Phil., Psychol. and Sci. Methods.* 1903, 122.

estimate of his reading-rate showed, however, that it dropped from an average normal of 10.67 words per second to 6.32 per second. There appeared to be an increase in writing rapidity just after a sentence was dictated with a gradual decrease in speed and increase in number of lapses as time went on before a fresh dictation. Lapses of letter-repetition and of omission of letters occurred with, also, substitutions of words from a preceding sentence. In one record a whole line was interpolated, made up, probably, from words persisting from preceding dictations. In part, this line seemed written without consciousness of writing it.

It was found that if the sentence dictated was in the form of a question, a definite "set" was aroused with considerable impulse to answer the question. We tried, therefore, a series of tests in which the reagent while reading to himself answered questions asked by the experimenter.

Both reagents reported that with practise it became progressively easier to maintain such a "double set." A, however, succeeded much more completely than D in carrying on a conversation. D, for the most part, wrote very simple and stereotyped answers while A produced elaborate sentences. For both reagents, the speed of reading fell considerably below the normal. A few words with reference to the reactions of each subject may be of value.

Five different tests at five different times were tried with A. His average reading-rate (words per second) under distraction was as follows: (1) 5.27, (2) 4.76, (3) 7.01 (answers relatively simple), (4) 5.72, (5) 4.86. His reading-rate was lower except in (3) than when he was simply taking dictation and much lower than his normal reading-rate. It is, however, a good speed in itself and seems remarkably rapid when taken in connection with the long and involved answers written in answer to the experimenter's questions.

Probably there was tendency for the reading to become more automatic than the writing. A reported that the reading became visual in places and that he got a good idea of the story but had little memory of details. Lapse of consciousness came when phrases or words were repeated while A was waiting for a new question. But, on the whole, there was fairly complete consciousness of what was being written. Breaks occurred when A was "tangled up" in the story. When he was very much interested in the story writing went easily. Many lapses of repetition or omission of letters occurred.

Five tests of the same sort were tried with D also. Her

average reading-rate (words per second) under distraction was as follows: (1) 4.17, (2) 3.02, (3) 4.49, (4) 3.8, (5) 3.21. Her normal reading-rate was about seven words per second. At first, as was said before, D's answers were very simple. She wrote "yes" or "no" if possible; otherwise, "Can't tell," "Don't know" or some other simple reply. As the task became easier with practise she attempted more ambitious answers. Lapses were much less frequent than in the case of A but certain stereotyped ones appear. Thus, "known" is constantly written for "know" without D becoming conscious of the error. "No one" is written for "nothing."

She reported that it was easier to put attention on reading than on writing but that she knew what she was writing. "More like split consciousness than unconsciousness." Answers to questions were first framed in inner speech but there was no need of vocalizing "yes" or "no." Hard questions inhibited both processes. In one place the question was heard indistinctly but answered correctly. In the last two tests there was much evidence of increasing automatism for the writing. D, while still conscious of the words written, lost consciousness of the individual letters. Once or twice she started an answer without knowing what she was going to write. There was less control and more spontaneity.

It is interesting to note that although an interval of more than three months intervened between the second and third test in this series, on account of the summer vacation, there was no loss in skill in manipulating the two processes for either reagent. We found, in general, that the practise-effect so noticeable in the work in the spring had held over when we returned to the experiments the following fall.

III. Silent Reading and Writing

Method. In this series of experiments the reagent wrote the given verse ("Thirty days hath September," etc.), continuously until he had finished reading the chapter of a novel. The experimenter shifted the sheets of paper so that only one verse was written on a sheet. The subject held the book conveniently in his left hand, with his right hand in position on the writing-pad on the other side of the screen. At a signal from the experimenter the writing and reading began, simultaneously so far as the reagent could manage it. At the signal for beginning the experimenter started two stop-watches. The whole time required for reading the chapter was obtained from

one stop-watch, the time required for writing each verse was obtained from the other. A and D served as reagents.

Toward the close of this series two normals were taken each day for the speed of writing the verse. One normal was taken with the screen under the same conditions as held during the distraction experiment, the other was taken without the screen under the usual writing conditions. In all normals the highest possible speed was maintained.

Normals for the rapidity of silent reading were also obtained at intervals. No attempt was made to test in detail the memory of what was read under distraction but the reagents were instructed to get the meaning. Both followed the story as in ordinary reading for pleasure without much difficulty. Their memory of the novels read is perhaps, after an interval of some months, more complete than is usually the case with casual reading.

The method here followed does not permit any exact equating of the amount read and written at any particular time. It was possible for reading and writing spurts to occur alternately. In the later work there is, however, a record of the amount read for each verse.

Results. The results are given in tabular form in Tables I and II. Table I summarizes the results for the reading of Mrs. Ward's "The Mating of Lydia" from the eighth chapter on. The reading of London's "The Abysmal Brute" and the reading of "The Game" by the same author were each completed in one long session and reading-normals were obtained for alternate chapters. The verses written while the reagent read "The Game" were done in ink, otherwise, the writing was in pencil.

A glance at the tables shows that A both wrote and read normally at a higher speed than did D. For both reagents there is an increase in the speed of the normals with practise. The normal with the screen was found to be slightly more rapid than that without so that the normals given with "The Mating of Lydia" are a trifle too high.

The normal reading-rate also shows a tendency to increase; such an increase was noticed for each story separately. The maximum speed is usually attained at the climax chapter.

The normal reading-rate under distraction shows a fairly constant increase, indicating the practise effect. The writing under distraction also shows increasing speed. D's average speed per verse dropped from 91.33 seconds to 63.66 seconds, A's, from 57.58 seconds to 41 seconds.

In order to assure ourselves of the simultaneity of the two processes, we hoped to get the average speed of both reading and writing under distraction down to the normal rapidity. The experiment, however, consumed so much time that we found it necessary to content ourselves with an approximation of the normal speed or with obtaining a record as low as the normal in an occasional verse. Toward the end of the series A's writing speed approximated the normal very closely. His record speed in this series for both normal writing and writing under distraction is 39 seconds. It should be noted that his reading-speed with the latter record is also high. (See Chap. VI, "The Abysmal Brute.") A's reading-rate under distraction also approached his normal. His introspective reports would seem to show that his reading became more and more visual as the test proceeded.

TABLE I

SILENT READING AND WRITING

Reagent	Date	"Mating of Lydia" Chap.	Pages[2] Read per Minute		Time of Writing Verse in Seconds				
			Normal	Distraction	Normal[3]	Distraction			No. of times written
						Av.	Min.	Max.	
D	Oct. 10	VIII	.....	.53	.....	89.38	71.0	116.0	16
	Oct. 13	IX	.....	.82	.....	91.33	83.0	107.0	15
	Oct. 15	X	1.62	.....	.....	.....	.....	.....	..
	Oct. 16	XI	.....	.78	.....	76.56	64.8	90.0	22
	Oct. 16	XII	1.40	.....	.....	.....	.....	.....	..
	Oct. 17	XIII	1.42	.....	.....	.....	.....	.....	..
	Oct. 20	XIV	.....	1.01	60.6	78.06	67.0	91.0	17
	Oct. 21	XV	.....	.84	63.2	79.54	64.4	94.0	20
	Oct. 23	XVI	.....	1.00	59.5	76.71	63.4	92.3	17
	Oct. 23	XVII	.....	1.38	.....	71.15	62.0	83.4	12
	Oct. 23	XVIII	1.66	.....	.....	.....	.....	.....	..
	Oct. 27	XIX	.....	1.14	54.4	71.51	62.4	84.2	17
	Oct. 28	XX	1.64	.....	57.0	.....	.....	.....	..
	Oct. 28	XXI	.....	1.13	.....	72.40	65.0	79.4	16
	Oct. 28	XXII	.....	.95	.....	72.36	66.0	85.4	23
A	Oct. 13	IX	.....	1.68	.....	57.58	52.0	62.0	12
	Oct. 14	X	1.95	.....	.....	.....	.....	.....	..
	Oct. 15	XI	.....	1.63	.....	51.02	40.0	57.0	15
	Oct. 15	XII	2.23	.....	.....	.....	.....	.....	..
	Oct. 16	XIII	1.88	.....	.....	.....	.....	.....	..
	Oct. 17	XIV	.....	1.75	.....	50.17	42.5	57.0	15
	Oct. 21	XV	.....	1.66	40.0	45.83	41.2	52.9	18
	Oct. 22	XVI	.....	1.80	42.5	48.87	43.6	54.8	14
	Oct. 23	XVII	.....	1.93	41.9	44.57	42.0	46.5	11
	Oct. 23	XXVIII	2.66	.....	.....	.....	.....	.....	.....
	Oct. 24	XIX	.....	2.20	43.5	47.62	44.5	59.0	13
	Oct. 27	XX	2.93	.....	.....	.....	.....	.....	..
	Oct. 28	XXI	.....	2.31	43.0	44.64	43.0	47.2	13
	Oct. 28	XXII	.....	2.21	.....	42.60	39.0	45.0	16

[2] About 275 words to the page.
[3] Without use of screen.

TABLE II

SILENT READING AND WRITING

Reading—"The Abysmal Brute."—(London)

Reagent	Chap.	Normal: Pages[4] per minute	Normal: Verse time in seconds	Chap.	Distraction: Pages[4] per minute	Distraction: Verse Time in Seconds: Av.	Min.	Max.	No. of times written
D	I	3.00	54.2[5]	II	1.30	66.65	61.6	78.6	15
	III	2.90	.....	IV	1.66	69.98	68.0	73.2	5
Nov.	V	2.61	.....	VI	1.84	71.52	65.8	81.0	16
11	VII	2.69	.....	VIII	1.60	71.91	68.6	73.0	7
	IX	2.19	.....	X	1.70	64.27	59.0	67.8	14
A	I	3.36	39[5]	II	3.43	42.2	40.2	44.0	9
	III	4.07	.....	IV	3.44	43.7	40.0	46.0	4
Nov.	V	3.97	.....	VI	4.23	41.0	39.0	47.5	12
6	VII	4.48	.....	VIII	4.67	42.2	41.0	43.8	4
	IX	5.05	.....	X	3.73	42.2	40.0	45.0	10
			Reading—"The Game"[6]—(London)—Ink Records						
D	II	2.84	61[5]	I	1.25	70.2	61.8	87.2	11
Nov.	IV	2.59	.....	III	1.43	63.66	60.0	73.0	6
20	VI	2.81	.....	V	1.32	65.66	57.0	71.0	12
A[7]	II	4.22	42[5]	I	3.92	43.75	43.0	44.5	6
Nov.	IV	3.30	.....	III	3.77	45.8	44.0	47.0	3
18	VI	4.42	.....	V	3.41	45.3	43.0	50.0	7

[4] About 149 words per page.
[5] With screen.
[6] About 132 words per page.
[7] Bad physical condition.

Introspections. The introspections (more accurately retrospections) given by the reagents are fairly adequate. Both reported, however, that any assumption of the introspective attitude in the course of the experiment complicated conditions badly. At times, in spite of concentration upon the reading, the thought would come "How am I doing this?"

For D another complication entered occasionally in that descriptive bits in the stories read suggested concrete visualizations. An attempt to maintain the three processes, reading, writing, and concrete imagery, caused considerable retardation. In time D was able to inhibit somewhat the tendency to visualization. She had also to inhibit a tendency to pause and think over any generalizations in the stories.

In order to make it possible to trace in some detail the course of the experiment we are giving extracts from the daily introspections chapter by chapter.

First, for D. "The Mating of Lydia," VIII—"All verse-words were mentally heard or vocalized, but as the reading

went on the vocalization became lighter, as it were." IX. "Auditory-vocal-motor cue for every word written. Sometimes this word seems to float over the reading consciousness. Complete loss of orientation of hand, very distressing." XI. "Process getting more automatic. Writing went easily. Auditory-vocal-motor cue for every word but not much kinaesthetic report except of a blurred sort." Certain stereotyped lapses appear as a doubling of the first stroke of "y" and the doubling of "v." D had the feeling of writing these letters correctly. XIV. "Writing went easily. The auditory-vocal-motor cue for verse floats over the auditory-vocal-motor reading. Lost report from 'y's' and 'i's'." Alignment lost completely after fourth record. XV. "Verse went easily after once started. Word by word cue. Toward close the kinaesthetic report began to lapse." A curious lapse appeared at this point. In writing the eighteenth verse D omitted the fourth line. In writing the next verse D repeats this line. D reported neither of these lapses. Toward the close of the nineteenth verse, D reported that she "came to" with a feeling that the word "April" had been written last, although at that moment she was writing another line. She concluded, incorrectly, that the second line had been cut. XVI. "The verse cue is auditory-vocal-motor, word by word. I believe that this word is mentally vocalized during breaks in the reading and that it then echoes in consciousness until written. Perhaps toward the close of each chapter synchronization of reading and writing occurs." XIX. "More automatic than before. Kinaesthetic report goes first. Feeling of irresponsibility as to details. Auditory-vocal-motor cue remains but seems to float over the reading. Sometimes there was an actual doubling of consciousness. Cue was more schematic than before. Meaning dropping out, numerals particularly unmeaning." XXI. "Reading becoming easier and easier to maintain, and writing more automatic. Writing cue persists but is more and more attenuated. Meaning evaporating from verse so that I can't hold my place. Kinaesthetic details gone." XXII. "The reading seems in part visual, in part, the two vocal-motor processes are synchronous. Meaning is evaporating; the lines and words are becoming detached. Lapse of kinaesthetic report very marked. Loss of orientation very distressing." D reported that she would not have been surprised to find lines omitted from the verses. None were omitted.

"The Abysmal Brute." II. "Lost space-relations. No idea where writing is." VI. "Lost connections between lines and

lost meaning of words even while writing them." VIII. "Lost connections within line, puzzled as to what has been written." X. "The writing for this chapter is much more AUTOMATIC than for any other chapter. Loss of strain in arm, usual blurring of kinaesthetic report. The first 'T' is now merely a signal for setting off reaction, it is not thought of as a 'T.'"

"The Game." I. "Distressing. Felt as though the pen were making no record. Kinaesthetic report inadequate and defective." III. "Easy smooth movement but lapsing of all kinaesthetic details." V. "Writing went easily. Verbalized verse throughout."

Secondly, A's introspections. "The Mating of Lydia." IX. "Consciousness of verse high at beginning, then lapsed. Vocal-motor cue throughout but this cue appears to be both marginal and imaginal." XI. "Vocal-motor cue throughout for verse but a rapid running together of words." XIV. "Lost consciousness of writing at one point in reading." "Came to" and found himself writing last line of verse. XIV. "Excessive spurting, excessive movement, many flourishes." XV. "Fairly conscious of verse with verbal cue; second, third, and fourth lines most apt to drop out. Consciousness reduced for these lines." XVI. "Exciting chapter. Vocal-motor verse-cue, line by line. Attention on reading rather than on writing." XVII. "Cue for verse is line by line, for each verse conscious of cue for only two lines, but these lines vary from verse to verse." XIX. "Chapter very interesting, main interest in story." XXI. "Attention on reading throughout. Can't tell about writing-cue. Perhaps a line-cue. Whole line verbalized. Writing and verbalization not synchronized. Very little attention on writing."

"The Abysmal Brute." II. "Cue as for normal writing. Hand report not detailed, only general movement felt." IV. "Attention largely on reading. Toward close of test writing may have become automatic." VI. "Chapter less interesting than before. Writing was less automatic." VIII. "The kinaesthetic report drops out of consciousness. If a word is pronounced, it feels written. General feel of movement is of up and down strokes. Chapter is intensely interesting. Climax of story." X. "Attention mainly on reading. Cue more pronounced at beginning than at close of verse. Probably unconscious of any cue on verse eight or nine."

"The Game." A was in bad physical condition, feverish. I. "Had a little trouble getting into the story because of the abrupt opening. Attention mainly on reading. The kinaesthetic report not detailed, only big sweeping strokes came to

consciousness. Reading was largely visual, a paragraph was taken in at a time with verbalization of occasional words only. No memory of any verbalization of verse, cue gone more completely than ever before, process more automatic." A found that the strain in his hand and arm and the pain in his head (due to condition) vanished from consciousness during the test. III. "Attention came to the writing, there were vocal-motor cues throughout, the reading was largely visual." V. "Interest in reading high. Emotional interest. Attention off writing but would revert to writing suddenly and unexpectedly. Less visual reading than before."

A reports, in general, in regard to the verbal cue for writing the verse, that only when he definitely thinks to notice whether or not it is present at the time of writing is he aware of it. Otherwise memory does not avail to tell whether or not such a cue was present.

IV. Simultaneous Writing and Adding

Method. This experiment was similar to those above, except that the adding of columns of figures was substituted for the reading. Instead of the verse "Thirty days," etc., the two verses beginning "Little drops of water," etc., were written.

The method of procedure was similar to that in the above experiments. The subject held the slips with the columns to be added in his left hand, while writing the stanzas with his right hand, extended through the black cloth screen. The experimenter, on the other side of the screen, observed the writing process and recorded the time. The subject wrote the two stanzas of this selection through once, adding as many of the columns of figures as possible. The experimenter took the time for writing the whole verse, for adding each column and for adding each problem, making note also of any breaks that occurred in the writing and noticing particularly the effect of announcing results.

Each problem added consisted of nine three place figures. The first 24 problems were taken from the Courtis Standard Arithmetic Tests No. 1. Later, problems were made by changing the order and transferring numbers given in the first 24. These later problems were prepared on an adding machine, the totals being turned under.

Only one subject was used in this test, Subject A. Subject D tried the test several times, but had such difficulty in performing the double operation that the test with her was abandoned. The experiment ran for twelve days, with, at

times, several days intervening between the tests. The longest break was five days, between the first and second tests. A normal writing record for the verse was obtained, both before and after each test, one without and one with the screen. The normals with and without the screen were alternated in order that neither normal might constantly have the benefit of the practise and warming up. On days that ink records were obtained, ink normals were used.

After completing the writing of the two verses, a very full introspective report was obtained from the subject. This introspective report covered the kind and amount of writing cue, the memory of lapses, confidence in the accuracy of adding, and such other things as seemed worthy of comment. At the conclusion of each test, the results of the additions were checked column by column.

At the beginning of the experiment, the subject was able to add only two problems while writing the stanzas once, then with a slight reduction in adding time, he added three problems (which tended to lengthen the writing-time of the stanzas) and then, after several more days, with a great decrease in the writing time, he was able to add two problems only. From the point of return to the two problems to the end of the experiment, only two problems were added during the writing of the stanzas, as both writing and adding times decreased almost simultaneously.

It was found necessary, in order to make the tabular representation of results comparable, to take two-thirds of the total writing time as the basis on which to draw up the tables, in those cases where three problems were solved.

Normal adding times were obtained on four different days.

Results. During the course of the experiment, the writing time for the two stanzas decreased from 122″ obtained on the first day to 46″ obtained on the last day. The highest adding time for all problems was 78″ on the third day and the lowest 18″ obtained on the last day. Both of these records were obtained for the first problem. For the second problem, the highest record was 60″ on each of the first, second, and third days, and the lowest was 24″ on the ninth day. For the third problem, the highest was 65″ on the fifth day and the lowest 35″ on the same day.

The average adding time under distraction for the first problem was 31.34″, for the second, 38.27″, for the third 48.60″. A third problem was utilized only a few times during the test, so that this high record is not significant. Throughout the experiment, however, the time for adding the second

problem is consistently higher than for adding the first, and the time for the third is higher than for either of the other two.

The average adding time for twenty-one problems under normal conditions was 19.8″ with a maximum time of 27″ and a minimum time of 15″. All of the normal additions were correct. Among the additions performed under distraction, we find that for the first problem 19 were correct and 33 wrong; for the second problem, 17 were correct and 33 were wrong; for the third problem 5 were correct and 10 were wrong; and for the fourth problem, 1 was correct, making in all, 42 correct and 76 wrong additions out of 118 problems. The number of correct additions made is indiscriminately mixed with the number of incorrect, except for one day, the fifth of the test, when eight problems out of thirteen were done correctly.

The average time for the normal verse writing, when the screen was not used, was 46.88″ and when the screen was used 45.41″. As before, a constant tendency for the writing with the screen (that is, when visual control was absent) to be lower than for the writing without the screen (when visual control was present) was noticed. The average normal time for writing the verse (speeded) with and without the screen was 46.22″. The highest normal writing, without the screen, was 53″ made on the 5th day and the lowest 42.5″ made on the 8th day. The highest normal time with the screen, was 50″ made on the first day, and the lowest 40″ made on the 12th or last day.

The following table, Table III gives in detailed form the course of the writing side of the experiment. The first two

TABLE III
WRITING TIMES FOR VERSE

Date	Normals		Distraction			
	Without Screen	With Screen	Times Written	Average	Minimum	Maximum
Nov. 26	50.5	50.0	1	122.0	.....	.....
Dec. 1	50.4	49.9	2	104.5	97.0	112.0
Dec. 2	50.0	46.0	4	96.75	83.0	121.0
Dec. 4	50.0	49.0	5	97.40*	86.0*	107.0*
Dec. 5	50.7	48.0	3	80.3*	75.0*	86.0*
Dec. 8	45.0	46.5	3	71.0*	64.0*	75.0*
Dec. 9	45.2	44.0	4	72.75*	66.0*	89.0*
Dec. 11	42.75	46.0	7	75.14	63.0	93.0
Dec. 12	46.8	44.0	5	63.30	52.0	78.0
Dec. 15	43.0	42.5	6	57.33	49.0	66.0
Dec. 16	44.0	40.5	7	54.21	49.0	63.0
Dec. 17	43.0	40.0	5	49.0	46.0	54.0
General Averages	46.78	45.53		78.64		

columns show the gradual decrease that was made in the normal writing times, and the last three columns the decrease in the writing times under distraction.

Table IV presents the results that show the gradual decrease in the adding times of the various problems. The last three columns give the general results of all the problems.

TABLE IV

TIMES FOR ADDING PROBLEMS

Date	Times Verse Written	1st Problem			2nd Problem			3rd Problem			All Problems		
		Av.	Min.	Max.	Av.	Min.	Max.	Av.	Min.	Max.	Av.	Min.	Max.
Nov. 26	1	59			60								
Dec. 1	2	51	47	55	52.5	48	57				51.7	47	57
Dec. 2	4	46	34	78	50.7	49	60				48.3	34	78
Dec. 4	5	35	31	40	51.6	34	60	60	65	53			
Dec. 5	3	34	27	40	40.6	35	50	44	49	35	39.5	27	50
Dec. 8	3	30	30	30	34.3	30	43	44.3	50	37	36.6	30	50
Dec. 9	4	40	28	53	33.2	31	37	41	44	39	38	28	53
Dec. 11	7	32.4	25	43	42.4	35	50				37.4	25	50
Dec. 12	5	26.2	25	28	35.4	24	50				30.8	24	50
Dec. 15	6	25.8	20	33	30.5	25	36				28.1	20	36
Dec. 16	7	23.1	20	28	31	26	40				27.0	20	40
Dec. 17	5	20.6	18	25	28.2	26	33				24.4	18	33
General Averages		35.3			40.9			47.3			36.6		

The next table V gives the results of the normal adding times. The results in this table are to be compared with those given in the preceding table for the adding times under distraction.

TABLE V

NORMAL ADDING TIMES

Date	Problems Added	First Problem			Second Problem			All Problems		
		Av.	Min.	Max.	Av.	Min.	Max.	Av.	Min.	Max.
Dec. 1	3	22.6	20.8	24				22.6	20.8	24
Dec. 9	2	16.0	15	17				16.0	15	17
Dec. 11	4	19.5	18	21	24.5	24	25	22.0	18	25
Dec. 16	12	17.3	16	20	20.6	17	27	19.0	16	27
Averages		18.6			22.5			19.9		

Tables III and IV indicate gradually increasing proficiency in handling two processes simultaneously. Inasmuch, however, as the record normal writing time for the two stanzas was 40″ and the record writing time under distraction was 46″ (both made on the same day) it is evident that the two processes never became completely synchronous. On the

adding side, the record normal adding was 15″ and the record adding under distraction 18″ (made on different days—16″ and 20″ were records made on same day). In the case of the writing the difference is 6″ and in the case of the adding 3″. The difference in the writing speed is the time taken to vocalize the verses.

Introspections. Extracts from the introspections made by the subject in the course of the experiment will serve to indicate the process gone through during the experiment.

First, a general summary of the introspections on the normal writing. These concern mainly the cue used for writing. "The cue is definitely vocal-motor, largely word by word, prolonged during the writing, the next word being verbalized just as the last stroke of the preceding word is being finished. There is a certain foreflitting of words that is too shadowy to serve as a cue. The unit for meaning is line by line, while the unit for the cue is word by word or, where words are very short, phrase by phrase. This makes it difficult to introspect the cue, as the meaning sometimes makes it seem as though it were coming line by line. When thinking about the cue, there is a tendency to cut the cue unit." From the beginning of the taking of normal writing to the end, a constant tendency for the verse cue to become less and less distinct is apparent.

The introspections concerning the cue made on the writing under distraction, are, in brief, as follows: first day,—"Verse verbalized completely, with the last words of each line verbalized a second time; for instance, the 4th line was first verbalized as a whole and then 'pleasant land' was repeated about the time I reached 'pleasant' in the writing;" second, third and fourth days,—"Vocal-motor cue for the verse;" fifth day,—"Cue for writing drops out in places. Writing report is not clear;" sixth day,—"Vocal-motor cue less noticeable than before. Grapho-motor report is lost;" seventh day,—"Vocal-motor cue definite but not very clear;" eighth day,—"Vocal-motor cue for writing more blurred than before. Some unawareness of writing but vocal-motor cue for verse throughout;" and ninth day,—"Vocal-motor cue for verse only at intervals. No remembrance of writing cue as a whole."

The introspections concerning the synchronization of the two processes are self-explanatory. They show, also, the gradual development of proficiency in handling the complex situation.

First Day. "Attention mainly on addition. Some flickering

of attention. Adding seems to be in part visual. Conflict between two activities is largely on a vocal-motor basis."

Second Day. "Two processes were not synchronous, although in the first verse slight tendency to synchronization is noticed. While writing, the adding was merely held in mind, except once or twice where small figures were added. Attention is largely on the adding. At certain places, doubling of the vocal-motor process seems to take place."

Third Day. "Attention more on adding than writing. Adding is in part visual, in part vocal-motor. Number of figures taken in by the eye at one time is growing. Habit of making additions without revision is forming. Writing comes in the adding breaks. Pause in adding comes after the first three numbers in the columns. Processes do not seem at all synchronous."

Fourth Day. "Considerable fluctuation of attention. Hand is never forgotten except when not moving. Adding seems more visual than before. At beginning of the column the writing and adding seem synchronous, then as adding becomes more difficult, the writing is stopped." (Experimenter's observation, "No writing of verse while A is speaking.")

General Introspection. "I start writing the verse first, giving a definite vocal-motor cue for the first line, then when the hand is on its way, start the adding, which at first runs along with the writing, both having a vocal-motor cue. The numbers are a mere flicker or shadow above the well-defined writing cue. But as the process grows more difficult, it tends to take over completely the entire vocal-motor process and the writing stops. When I get to the end of the column, the adding being over and the result announced, I give a second definite vocal-motor cue for the writing. Again the two processes run on synchronously for a while, with the adding gradually taking up the entire process. When I get through adding the first problem, I become conscious of having written only a small part of what should have been written, and spurt, paying a little more attention to the writing than to the adding. Then again the adding comes to the fore."

Fifth Day. "Synchronization never occurs for more than half a column. Attention is largely on the adding but there is a little more synchronization than before. Attention fluctuates. Practise effect is beginning to show on the adding. Adding has become more visual. Writing half drops out in places."

Sixth Day. "Processes are more synchronous than before. Adding is becoming visual. Much synchronization. Writing tends to drop out and attention goes to the adding."

Seventh Day. "Very difficult to-day." 1. (first writing time). "Processes don't synchronize." 2. "Slight synchronization." 3. "Processes synchronized throughout the first verse and first two lines of second. Visual adding is not noticeable. Unit of addition is growing bigger."

Eighth Day. "Processes well synchronized. Writing and adding went on together. Some unawareness of writing. First problem was well synchronized with writing."

Ninth Day. "Attention chiefly on adding. Some fluctuation of attention. Lapse of hand-awareness for long stretches. Hand innervation comes more easily than earlier in test."

Tenth Day. "Went very easily. Synchronization throughout. Adding seems a trifle more visual than before. Large adding units are taken in at a glance."

Eleventh Day. "Very little attention on the verse." (Experimenter's observation, "Results now announced while A is writing.")

V. Reading Aloud and Writing

Method. The conditions under which this test was tried were, for the most part, similar to those of the third section except that the reagent read aloud instead of reading silently. In order to test the memory of what he had read the reagent stopped writing at the close of every verse, or, on some occasions, at the close of every third or fourth verse and gave what he could recall of the contents read. He also gave his introspections and reported any lapses in the writing that he remembered. Breaking the writing at frequent intervals made it possible to determine the exact number of words read during any particular period, and insured a fairly accurate equating of the amount read and the amount written at any particular time. In this respect the records are more satisfactory than those of the second section. Moreover, since the introspections were given at shorter intervals they are probably more adequate than before, although the frequent breaks may have prevented a gradual absorption in the reading and contributed to a heightened capacity for handling the double process through extreme concentration of attention for a short period.

As before, the speed with which the verse was written was timed. Normal reading-rates were obtained at the beginning and toward the close of each session. Normals for writing speed we obtained one at the beginning, the other at the close of the session, alternating as first or second the normal written with the screen and the one written without the screen.

The experiment ran for twenty-three sessions, Subject A served as reagent.

Results. The growing ease with which A handled the situation is indicated by Table VI which shows, first, the increase in the number of words read per second, and secondly the gradual increase in the rapidity with which the verse was written; a rapidity, toward the close of the experiment, approximating the normal writing-time.

TABLE VI
READING ALOUD AND WRITING. REAGENT A

Reading	Date	Words read per second				Time of writing verse				
		Normal	Distraction			Normal[8]	Distraction			No. of times written
			Av.	Max.	Min.		Av.	Min.	Max.	
"The Intruder"	Dec. 18	No record	2.31	2.90	1.92	39.0	71.21	49.5	111.0	7
Scientific reports	Dec. 19	"	2.43	2.73	2.03	38.5	63.83	56.0	72.0	6
"	Jan. 12	"	1.67	1.98	1.48	41.0	80.37	72.5	90.0	4
"	Jan. 13	3.33	1.74	2.36	1.35	40.5	63.50	57.0	66.5	10
"	Jan. 14	3.38	1.83	2.12	1.50	39.5	61.90	60.0	65.0	5
Phillip's "Genesis of Number Forms"	Jan. 15	3.38	2.16	2.55	1.81	42.0	62.35	56.0	71.0	10
	Jan. 16	3.45	2.38			41.0	58.00	53.0	72.0	8
	Feb. 3	3.55	2.27	2.58	1.95	41.0	64.29	56.5	71.4	7
	Feb. 4	3.33	2.03			40.7	55.29	44.0	67.5	8
Sanford, "Psychic Research in the Animal Field"	Feb. 5	3.34	2.39	2.65	2.13	38.4	55.09	50.0	61.0	9
	Feb. 9	3.53	2.15	2.40	2.01	40.0	55.73	51.2	58.1	7
	Feb. 10	3.91	2.48	2.59	2.36	36.0	51.55	48.0	54.0	11
"	Feb. 11	4.00	2.84	3.13	2.53	38.4	46.65	43.2	52.0	7
"	Feb. 13	3.76	2.61	2.91	2.30	35.5	47.78	43.0	55.5	6
"	Feb. 16	3.63	2.58	2.96	2.36	38.0	49.71	44.7	53.6	6
"	Feb. 17	3.83	2.85	3.03	2.40	33.7	44.65	42.4	46.5	7
"	Feb. 18	4.13	3.21	3.55	2.91	36.7	43.50	40.7	46.8	6
"In the Wireless Room" (story)	Feb. 19	4.26	3.07	3.43	2.84	35.8	47.95	45.0	52.5	6
	Feb. 20	4.03	3.54	4.25	2.89	36.0	43.57	40.1	45.4	7
"	Feb. 24	4.25	3.24	3.83	2.65	38.0	43.15	40.5	46.0	8
"	Feb. 25	4.17	3.34	3.89	2.80	34.2	41.12	37.1	45.4	9
"	Feb. 26	4.71	3.75	4.00	3.32	36.1	39.19	35.0	43.8	9
"	Feb. 27	4.91	3.62	4.01	3.13	33.0	37.96	34.5	45.5	5

[8] Speeding with screen.

Introspections. On the introspective side there was reported a gradual increase in the ease with which the double process could be maintained. At first there was very little synchronization of processes. Breaks would occur in the reading, during which A would get his vocal-motor cue for the writing. These fluctuations in attention were very rapidly and skilfully executed. Frequently, A spoke of getting a general "set" for the two processes before beginning writing or reading.

A bad initial "set" caused breaks in the process, so, too, any interference while the writing was in progress. Such interference occurred when there was difficulty in getting the

meaning of what was read, when there was difficulty in pronouncing a word, when the paper slipped however slightly, or when the experimenter touched A's hand. On several occasions the entrance of someone into the room where the experiment was being conducted caused bad breaks in the process. These breaks were evidenced not only by A's report but also by the slowing of the time of writing and, usually, by a repetitionary lapse. Such breaks show also the tension created by the situation. Greater ease in writing the first three lines seemed due to persistence of initial set through that point. After that there was tension.

At the fifth session A reported that the kinaesthetic "report" was lapsing. From this time on, there were reports of increasing ease in handling the situation, there was also increasing excellence in the memory of reading-contents. A's reliance upon the kinaesthetic report was shown by the difficulty he had in handling the second and third lines, both of which commenced with a capital a. A repeated feeling of doubling or omitting one of these lines seemed so determined.

The experiment ran a long time before A reported any synchronization of the two processes. The growing ease in handling the situation seemed due, not to synchronization, but to the fleeting character of the vocal-motor writing cue. In time, too, the cue was cut in that one word or letter could serve as the cue for several words. Not until the thirteenth session did A report synchronization of processes. He began to describe the writing cue as floating over the reading. In the course of this session the writing became "fast and furious." At the next session A began to complain that meaning was evaporating from the verse he was writing, so that if a break occurred he had difficulty in picking up his cue. Later still he reported, "Cue seems to come after not BEFORE the word is written. It is definitely on top of the reading process." Again, "The two processes are definitely synchronous. Lines seemed shoved together, telescoped. Not enough words. What's gone?"

The reading varied from expressive reading to reading that was very monotonous or breathy. With a shift in style, as one article was changed for another, there was for a time increased difficulty in handling the problem which indicated that in part attention-habits, induced by familiarity with style, were operative in increasing speed of reading. The normal reading-rate also showed a progressive increase, not only within the scope of a single article, but also in the experiment as a whole.

The final experiments in this section were carried on while A was reading a very interesting story. Toward the climax of the story there was a strong inclination to stop writing altogether.

VI. Reading Aloud and Writing from Dictation

Method. There was little change in method. As before the reagent sat to the left of the screen with his arm, extended through the sleeve, resting on the writing-pad, while the experimenter, on the other side of the screen, manipulated the writing-sheets, dictated material, and timed the reading and writing. The reagent read aloud continuously from a story while writing from continuous dictation, words given, that is, one at a time as rapidly as the reagent could write them. At the close of set intervals, varying from one to three minutes, with an occasional five minute period, the reagent was stopped and the experimenter recorded (1) the subject's memory of the contents read, (2) his memory of the matter dictated, (3) introspections as to the course of the experiment. In the second half of the experiment his memory of the contents dictated was recorded first and his memory of the contents read taken after that. Record was made of the number of

TABLE VII
READING ALOUD AND WRITING FROM DICTATION
Reagent D

Story	Date	Normal	Distraction			
		Average number of words read per second	Average number of words read per second	Average number of words written per minute	Average number of words remembered	Length of writing interval
R. H. Davis' "In the Fog"	Mar. 30	4.03	3.07	20.00	3.0	1 min.
	Mar. 31	3.83	3.18	19.85	3.1	1 min.
	Apr. 1	4.86	3.46	16.25	1.5	3 min.
	Apr. 2	4.75	3.63	18.22	1.4	3 min.
	Apr. 3	4.91	3.98	17.44	2.1	3 min.
	Apr. 6	....	3.53	15.88	1.7	3 min.
	Apr. 7	5.00	3.86	17.90	.9	3 2(5)
	Apr. 8	4.50	3.67	15.94	.7	3 4(6)
Davis' "A Derelict"	Apr. 27	4.08	3.13	18.58	1.2	3 min.
	Apr. 28	3.83	3.32	16.73	1.1	3 min.
	Apr. 29	4.01	3.43	18.06	1.2	3 min.
Davis' "La Lettre D'Amour"	Apr. 30	4.18	3.76	18.94	.9	3 min.
	May 4	4.95	4.00	20.77	1.6	3 min.
Davis' "The Man With One Talent"	May 5	5.18	3.98	17.83	1.0	3 min.
	May 6	5.76	4.60	21.22	2.7	1 min.
	May 7, a.m.	5.35	4.92	22.85	3.4	1 min.
	May 7, p.m.	5.65	4.66	22.33	1.3	1 min.

words read aloud and of the number written from dictation each minute. At the beginning of the session and sometimes toward the close a record was made of the normal rate of reading aloud. Both A and D served as reagents.

Results. As before, the tables (VII and VIII), show the effect of practise upon the reading-rate. There is a fairly constant increase in speed for both the normal reading and that under distraction. The introspective records show that subjectively, the reagents found the experiment an exceedingly easy one to carry out but in spite of this the reading with distraction failed to reach the normal speed. The number of words written per minute[9] is well under that which can be taken in normal conditions, and, curiously, there is little evidence of any tendency to an increase in the number of words taken per minute. A's record would seem to indicate a tendency to decrease, D's record at the point of greatest reading speed is higher than the average. Throughout, the record for the first minute is consistently higher than that for subsequent minutes, as though during the experiment the reagent "ran down." Both reagents had a good memory of the contents read but A's report was considerably more detailed than D's. There were times of curious lapse of memory of details, both reagents reported a feeling of having them just on the tip of the tongue and then losing them. A's memory of the

TABLE VIII

READING ALOUD. WRITING FROM DICTATION

Reagent A

Story	Date	Normal	Distraction			
		Average number of words read per second	Average number of words read per second	Average number of words written per minute	Average number of words remembered	Length of writing interval
Davis' "The Bar Sinister"	Mar. 2	4.66	2.28	22.7	9.1	1 min.
	Mar. 3	4.02	3.20	19.8	9.4	1 min.
	Mar. 5	?	3.26	25.0	7.0	1 min.
	Mar. 6	4.58	3.19	19.3	1.0	3 min.
	Apr. 9	4.66	3.88	19.5	1.5	3 min.
Downey's "The Ghostly Sting"	May 7	4.60	3.70	21.0	7.1	1 min.
	May 8	4.40	3.65	19.2	10.0	1 min.
	May 11	5.3	4.48	17.5	6.1	1 min.
Davis' "The Editor's Story"	May 12	5.15	4.02	18.3	6.0	1 min.
	May 14, a.m.	5.00	4.46	17.2	5.2	1 min.
	May 14, p.m.	5.06	4.49	17.6	5.0	1 min.

[9] The number of letters written per minute was also recorded, but to both reagents the word appeared to be the actual unit.

words written was much more complete than D's. It is significant that just about as many words could be recalled at the close of a one minute interval as at the close of a three minute or longer interval. D lost, or failed to make, connections between words much more significantly than A. On one occasion when the experimenter dictated an interesting bit A repeated the whole (twenty-one words), without a mistake, at the close of the writing. He even criticized the use of certain terms by the author of the story. It is true that his reading-rate on this occasion was somewhat retarded and his memory of the contents less complete than usual. When the experimenter tried dictating a sentence twice, A noticed this and gave many of the words that had been redictated. D, on the other hand, failed to recognize the dictation of such a familiar line as "My country 'tis of thee," a failure which clearly evidenced the absolute lack of connection between the words dictated.

Introspections. Both reagents found this task a very simple one, requiring little effort, but from the first there were some differences in their reports. D failed completely to make connection between the words dictated although reporting that while writing each word she knew, with rare exceptions, what it was. At the close of each interval she seldom recalled more than four or five isolated words. A, on the other hand, in the earlier sessions remembered many more of the words dictated, there were occasions when he had an exact memory of both what was read and what written. Moreover, in the first tests at least, he caught the drift of the story that the experimenter was dictating to him.

At first, A repeated after the experimenter the words that were dictated, later such a tendency to repetition dropped out, on some occasions a verbal flicker (vocal or imaginal) seemed to follow the dictation. D reported no such tendency to repetition of words dictated but noted occasionally a faint visual flicker of a word written. Both subjects reported that the experimenter's voice sounded faint, far-off, muffled, even when the experimenter was speaking very loudly and emphatically.

Throughout these tests A reported, "I get the words, as they come, in their connections but they are lost very quickly." Toward the close he caught only phrasal connections. As A reached the climax of a story there was loss of writing control and failure to get the connection between words but the writing did not become completely automatic. When greatly interested in the story there was some tendency for A to stop

writing, the experimenter had to dictate very loudly and at times had to dictate a word several times with great emphasis before it would be taken. Sometimes A reported that a word was heard and written, and that was all there was to it. There was, however, a tendency for him to become alert to the writing. This occurred when the subject-matter especially interested him.

D's reports are more important from the standpoint of automatism. A few extracts from her reports may be given. March 30. "No idea what the story dictated is about." March 31. "Part of the time a feeling of divided attention. Sometimes, half lose the experimenter's voice." April 1. "Words written fuse with story. Once or twice words were written automatically. Difficult to introspect. Lost consciousness of hand once or twice. Extraordinarily little consciousness required for writing. Words dictated often call attention to the writing, so too do any unusual words." April 2. "Lost consciousness of writing. Just get voice." April 8. "There are times when writing is pretty nearly automatic, times when I do not know what words are written." April 27. "Drop in kinaesthetic report. Feeling of irresponsibility as to details." April 28. "Very little writing consciousness but I know the words while writing them. Sometimes lost meaning of word dictated. In places a tendency for the reading to become automatic." April 29. "Words have no meaning. Wrote a few words without return report from hand." April 30. "Feeling of complete uncontrol. (Writing is indecipherable.) Forget words while writing them." May 4. "Can't spell words dictated, they vanish too soon." May 6. "Tendency to forget words in middle, memory seems to hold over about four letters." May 7. "Heard word and wrote it, nothing more. Some very easy words seem to be written unconsciously."

VI. A Supplementary Experiment

Silent Reading and Writing from Dictation

Because of the fact that our reports showed so much more evidence of conscious control than did the report of Solomons and Stein[10] under somewhat similar conditions, we tried a supplementary test in which we attempted to conform to their conditions. We substituted silent reading for reading aloud, we gave up speeding, we let the writing run on for long inter-

[10] L. Solomons and G. Stein, Normal Motor Automatisms, Psychol. Rev. III., 1896.

vals until fatigue set in, we slowed the rate of dictation, instead of making it continuous, between dictations we fell back on meaningless strokes, we no longer called for reports on the memory of what had been read. No significant change occurred, however. The experiment simply took less effort than before. Although D continued such writing for nearly two hours (two sessions) there was no report of greater automatism than in the preceding test. On the last day, when she was feeling unusually fit, she remembered more of the words written than ever before, recognized redictated sentences, and that without the slightest consciousness of effort. The most significant lapses under these conditions occurred in the case of A, as follows: first, his failure at times to take a word until it had been dictated several times; second, long retardations in taking words without, however, a redictation; third, the repeated writing of a word although it had been dictated but once, because, as he explained, he could not remember whether he had written it at all.

VII. General Summary and Conclusions

1. *Normal Rapidity*

As the experiment continued there was a continuous increase in the normal rapidity for all operations tested, so that the question arose whether we could count on having ever reached a permanent record. This increased speed in the case of reagents already practised in the operations in question is interesting. It depended undoubtedly upon the effort put forth, the definite intention to speed to our limit. As Book and other investigators of the learning process have shown, effort of attention makes for greater efficiency.

Writing Normal. For reagent D the time required for writing the first verse ("Thirty days," etc.), fell from 60.6 seconds to 54.2 seconds. These normals were taken without the screen and in pencil. They represent little practise except in so far as the writing of the verse under distraction served to lower the normal time record. This is a good speed, equivalent, for the verse written, to more than two letters per second.

For reagent A we have at hand a much greater number of records both with and without the screen. The slowest record without the screen for thirty trials, on thirty different days, was for the first verse 44 seconds; the fastest, 33.5 seconds. With the screen, the maximum was 42 seconds and the mini-

mum 33 seconds. The average of the last six trials with the screen was 35.5 and without the screen 36.7. These records are very rapid ones, from over three to more than four letters per second. The best records would be made unexpectedly and then, in time, this record would become an average one. Such increase in efficiency even after the limit would seem to be reached is common to all practise curves. It is probable that A reached his physiological limit in speed; he had long passed the limits of legibility. The verse-cues served to release a series of movements that were cut in every possible way. A tried, deliberately, methods of shortening his time; he tried, for instance, the effect of decreasing the space between lines, diminishing letters, etc. The fact that the record with the screen stayed pretty constantly below that made without the screen shows the curious inhibition from the visual report even before the limits of legibility were passed. With the second verse written we have again an increase in speed with practise, with a higher speed achieved with, than without, the screen.

Reading-normals. D's rate in silent reading estimated from thirteen normals varied from 6.3 words per second to 7.4 words per second. A's, estimated from the same number of normals, varied from 8.6 to 12.5 words per second. Both records are good (A's unusually so) if we may judge by comparison with the reading-rate for twenty-eight persons given by Huey, who writes, "When the silent reading was at maximal speed, the rates ranged from 3.5 to 13.5 words per second." [11]

The reading-speed, as the writing-speed, showed a tendency to continuous increase, but this tendency was complicated by two other factors: (1) The effect of the contents read upon the speed of reading, with great rapidity at points of climax, and (2) the effect upon reading-rate of familiarity with a writer's style.

The general tendency toward increase in reading-rate seemed in D's case due to inhibition of certain general habits. Many things have operated to reduce D's speed in reading such as close grammatical work on the classics, critical reading, correction of students' themes. She developed during the practise some power of inhibition of a habit of criticizing details and pausing over generalizations. She also inhibited, in part, a tendency to indulge in concrete visualization of descriptive bits. A's increase in speed would seem due, in

[11] "The Psychology and Pedagogy of Reading," pp. 174 f.

part at least, to an achievement of a certain amount of visual reading. If we may trust A's introspections he handled the double process to a certain extent by getting the meaning of what he read visually and thus releasing the articulatory process for control of the writing. Pintner[12] has shown that, with practise, reading without articulation may be acquired when the articulation of what is read is artificially inhibited. He reports, further, " That practise in reading without articulation tends to aid ordinary reading, most probably by shortening the habitual visual reading." In the same way A carried over into the normal reading habits acquired during the reading from distraction.

As was said before no effort was made to test the memory of what was read but both reagents followed the story with interest.

The rate for reading aloud was obtained on sixteen different days for D. This ranged from 3.83 to 5.65 words per second. For A we have thirty normals obtained on as many different days. His rate ranges from 3.3 to 5.15 words per second. Such reading rates are excellent. To quote Huey again,[13] " In reading aloud, the average of the slowest reader was 2.2 words per second and that of the fastest 4.7, at the ordinary rate, and at maximal speed the corresponding range was from 2.9 to 6.4 per second."

The tendency to progressive increase in the rate of reading aloud is perfectly evident although, as before, interest in contents and familiarity with an author's style affected speed. D's record, given in connection with the dictation experiment (Table VII), shows that between the eighth and ninth records a break of more than two weeks occurred. Following this break there is a series of low records. These low records may be due to the break in practise but, more likely, are to be attributed to the bad physical condition of D at the time. With improvement in condition, May 30, there came rapid improvement.

The increase in the normal rate of reading aloud was due very largely to improved methods of breathing. Both reagents lowered their voices in the later experiments and there was some tendency to blur enunciation. D's enunciation is more distinct than A's. Relatively her reading aloud is much more rapid than her silent reading. It is difficult to explain this

[12] " Inner Speech during Silent Reading," Psychol. Rev. XX, 1913, pp. 129-153.
[13] *Op. cit.*, p. 175.

unless the sound of her own voice acts as a spur to attention. This may well be the case as D is unusually sensitive to auditory stimulation. A, on the other hand, pays no attention to the sound of his own voice and finds that reading aloud disturbs the tendency to visual reading. A found at times a tendency to glance ahead and get the meaning before resorting to the slow reading aloud.

D was able to verbalize mentally the test-verse more rapidly than could A. Possibly the fact that D's inner speech is auditory-vocal-motor and that A's is vocal-motor accounts for this. The range for A (14 trials, different days), is from 3 to 6 seconds. For D (6 trials), from 2.5 to 3 seconds.

It is obvious that the progressive increase in the speed of the normal operations introduces some difficulty in interpreting the results under distraction, for a record under distraction that approximates the normal for the day may yet be far from the normal that might be reached under sufficiently high concentration of attention. Only if a fairly constant normal were attained, one obviously approximating a physiological limit, would we have at hand a satisfactory standard for checking the rate under distraction. In the time at our disposal it was, however, impossible to carry the experiment out to such an extreme. A's writing record on the first verse (33 seconds) may, however, be taken as a satisfactory limiting value. The final normals for reading aloud must also be very close to the physiological limit for the readers in question as evidenced both by the effort involved in the spurting and by the tendency to slur words.

2. *Rapidity under Distraction*

In the course of the discussion we have already indicated the general bearing of distraction upon rapidity. In the first place we find with practise a progressive increase in the speed with which reading and writing (or reading and adding) could be carried on. All the tables could be converted into characteristic learning curves. A consideration of the bearing of this fact upon the general interpretation of results will be attempted later.

Let us now summarize the facts recorded relatively to the speed of work under distraction.

In the test on simultaneous writing and silent reading there were several instances where A's most rapid rate of writing for the day was within a second of his normal record, or, more correctly, since the normal was taken without a screen, within two seconds of the normal. There are cases where

the most rapid record is the same or lower than the normal and the average time only slightly (two to three seconds) above the normal. Toward the close of the tests, the mean variations on the time-records were very low. There were also some very high reading-rates. Thus, in reading Chapter VIII of "The Abysmal Brute" under distraction A reached his second highest speed record for that story, a very high speed, something over eleven words per second. No doubt A's ability to read visually served the purpose of releasing the vocal-motor process for control of the handwriting. The average writing speed for this day is within three seconds of the normal. Since A's writing-habits are exceedingly well-organized and smooth, the only thing needed for maintaining the writing of the verse was a schematic and rapid vocalization of a few words. To this we must add also a capacity for extending the scope of attention.

D's writing-speed under distraction approached the normal less closely than did A's. The best records are found in Table II. For "The Abysmal Brute" the best average is ten seconds higher than the normal, and the record speed under distraction at least 4.8 seconds higher than the normal. In "The Game," the best average is about three seconds above the normal but there are one or two individual records below the normal. Very great retardation is evident for the poorest records. On account of her slower reading D wrote the verse a much greater number of times than did A but in spite of this failed to reach a constant reaction as soon. Had the experiment been continued with her she would have reached, probably, a more constant reaction. Her writing-habits are, however, much less mechanized than A's and her span of attention narrower. Under distraction her reading-rate in no case approximated the normal.

Let us turn now to A's record for writing while reading aloud. Between this experiment and that of silent reading the adding test intervened so that more than a month's time separated the two reading tests. In order to make the two series of records comparable the verse written in the silent reading test was again employed. It is interesting to note that the normal time for writing this verse began in the later series well towards the lower limit of the earlier series. The retardation of the speed under distraction shows, however, that reading aloud while writing, is a very much more difficult thing for A than is simultaneous silent reading and writing, no doubt because of the inhibition of visual reading.

In part, however, at least toward the close of the series A still had recourse to visual reading since he would glance back to take in the meaning, then resort to automatic vocalization of the words read while concentrating for a short period on mental vocalization of the verse. The initial record in this series is much higher than that in the first series. The experiment ran nine days before A reached an average writing speed as rapid as that reached on the first trial with silent reading. Toward the close of the series he made, it is true, an even better time-record under distraction than in the preceding series since once his verse-time fell to 34.5 seconds as against 39 seconds in the other. During the course of the experiment the normal time had, however, been constantly falling, so that the best record under distraction is 1.4 seconds above that day's normal record, while the best averages are from three to four seconds above the normal. A much greater variation in speed than occurred in the preceding series is also evident. The worst records show considerable speed retardation. A's reading-rate during writing is in this test considerably retarded, relatively much more so than the writing speed. There is, even in the fastest records, a loss of about a word per second. It would seem that the conditions of the test introduced a certain amount of vocal-motor conflict. The inner verbalization of the verse required from three to six seconds for A, hence a retardation of reading-speed from twelve to twenty-four words every verse-time (something over thirty seconds) would serve to keep the processes going. The retardation of reading speed is just about what would be required. Throughout, A's memory of what was read was excellent.

In simultaneous writing and adding A's writing-speed is retarded six seconds at least, and the adding speed at least three seconds. It is interesting that again the retardation in adding should be just about that required for thinking the verse.

In the experiment on taking dictation while reading aloud neither reagent approximated the normal reading-rate and that too in spite of the fact that both reagents felt the task to be a fairly simple one and made every effort to speed the reading.

3. *Graphic Lapses*

A careful tabulation was made of the graphic lapses occurring during simultaneous writing and reading and much material has been collected for subsequent study. At present

we shall confine ourselves to summarizing the general observations.

D's lapses during silent reading and writing were surprisingly few in number but significant in character. Often verses were written without a single lapse. At most, if we omit line-lapses, four lapses per verse occurred. The lapses that appeared were stereotyped. Such a stereotyped lapse occurred in the doubling of "v" and the repetition of the first letter-stroke of "y." The kinaesthetic report for these letters was absolutely lacking. Even after D was informed of such a lapse and attempted to inhibit it, it kept reappearing. This loss of kinaesthetic report on certain letters is curious. At times D reported, incorrectly, that she had dropped lines. Such omission actually occurred on occasion. Quite the most significant lapse that was noticed throughout the whole experiment for either reagent was the omission of a line in one verse with its repetition in the verse immediately successive (some sixty seconds later) without D's knowledge of either lapse.

A gave a great number of lapses for every verse. In the experiment on writing while reading silently these lapses consisted for the most part of lapses of omission such as the cutting of strokes, the omission of letter-parts or of letters and words, lapses similar to those which occur in his normal rapid writing, probably indicative of anticipations on the part of attention which keeps ahead of the writing. Line-lapses of omission or repetition were usually reported by A at the close of the verse. The number of omissions of letter-parts and of letters gradually increased throughout this series as a whole. Many omissions became stereotyped and a given word presented a progressive cutting of strokes. Thus "February" appears successively as "Febrary," "Febray," "Febry" and "Febu." Toward the close of this series of experiments repetitionary lapses and lapses of substitution began to appear. There occurred also a curious exchange of letter-parts, a lapse purely motor in origin.

With the shift from silent reading to reading aloud a most interesting change occurred in the kind of lapses that were most frequent. The repetitionary lapse appeared again and again. This lapse evidences the attentional conflicts and, perhaps, also some difficulty with the kinaesthetic report. An exchange of letters as well as of letter-parts appeared; thus, "April" became "Aprli." Hybrids, due to fusion of two words are found; thus, "gives twenty" becomes "gventy." The repetitionary lapses showed a tendency to become stereotyped as had the omission-lapses in the preceding series.

On the fifteenth day of this experiment the repetitionary lapses began to drop out noticeably and the old lapses of cutting strokes and omission of letters reappeared. Such a shift in kind of lapse evidences, as did the introspective report and the increased rapidity of writing, the growing skill with which attention handled the double process.

The shift from repetitionary lapses to lapses of omission in this second series is more easily understood than the tendency in the simultaneous writing and silent reading to shift toward the close of the series from lapses of omission to repetitionary lapses. Possibly in this later case the distraction has become so complete and the verbalization of the verse so cut that attention to the writing came only in jerks and at very infrequent intervals. The experimenter found that she could produce these lapses of repetition almost at will (without A's knowledge of the occurrence) by the simple device of moving the paper gently or of lightly touching A's hand. Such artificial production of lapses promises much in the way of future understanding of them.

Throughout the course of the experiment A was able by thinking back over the writing to recall many of the lapses made with surprising accuracy. There were times, however, when he reported a strong feeling of having made lapses which were not found to occur.

4. *Simultaneity of Processes*

We are ready now to raise the question as to the actual *simultaneity* of mental processes during the course of the experiment. Simultaneity of motor reactions occurred. A was able to continue writing, even to start a new word while announcing the results of mental addition, and able to write while reading aloud. It was found, however, that even with the immense amount of practice reported the verse could not be written absolutely mechanically, simply by an unwinding of the movements; some slight control in mental terms was necessary. The mental verbalization of the first verse employed took from three to six seconds—that much retardation of reading-speed would probably serve the purposes of control. We have already indicated that just about so much retardation occurred in the reading-rate under distraction. There is, possibly, one exception, the rate of silent reading of Chap. VIII of "The Abysmal Brute" where A achieved an extraordinarily high reading speed with very little retardation of the writing speed. Here we may have a bit of evidence for actual simultaneity of the mental processes.

Introspectively, we get reports of actual synchronization.

A reported that while he was reading aloud an imaginal vocal-motor cue floated over the overt vocalization of the words read. It is obvious, however, that such a report must be checked by objective records. Still it is possible that the slight retardation in speed noticeable throughout the tests is a general retardation due to the amount of effort needed for keeping the two processes going rather than a specific retardation of either of the operations. The normals were taken with great effort of spurting and the attempt to drive two processes to the limit was very fatiguing.

Two interpretations of our records are, therefore, possible. The first is that two operations may be carried simultaneously with slight loss of speed if one be so mechanized as to require little sensory control; the other is that two mental operations may for a short period be actually synchronized but with a slight loss in efficiency due to the demands upon the general mental (nervous) energy.

The question of speed under distraction is of interest not merely as a check upon the subjective impressions of the reagents but also in connection with certain reports that have been made upon automatic writing. Thus Solomons and Stein write in their report upon "Normal Motor Automatism:" "In all automatism the tendency toward increased speed is marked. Writing tends towards a pace that very quickly tires, reading towards a rapidity that prevents distinct articulation, dictating towards a speed that soon becomes hopelessly fast for the writer."[14] In the above report no records are given of either reading or writing speed so that it is impossible to tell the precise data upon which such a conclusion is based. In our experiments we failed to attain, except in rare instances, an approximation of the normal possible speed. This failure was particularly true in the case of the dictation experiment which resembled rather closely the kind of experiment carried on by the writers referred to above. It is quite true that, subjectively, we had often a feeling of "furious" speed and in the verse-writing tests there are samples of writing showing, at least in the case of A, excessive and flourished movements, but, on the whole, there is evident speed-retardation of some sort.[15]

[14] Psychol. Review, III, 1896, p. 507.

[15] Probably a greater rapidity than that of normal writing without speeding does result for certain individuals under distraction. The excitement of the distraction serves to introduce considerable tension; each operation serves as a spurt to the other. But the limits of the NORMAL SPURTED SPEED were not reached, in our experiments at least.

5. *Lapses of Awareness*

Again, in the matter of lapses of awareness our reports are in certain instances different from those of Solomons and Stein who distinguish two forms of lapse, lapse of memory and "real unconsciousness." They write,[16] "The consciousness without memory seems to *approach as its limit,* simply a condition in which the subject has not the faintest inkling of what he has written, but feels quite sure that he has been writing. It shows no tendency to pass beyond this into real unconsciousness. It seems to depend on the lack of associations between different words. And conversely real unconsciousness appears not as a final stage of a gradually decreasing memory, but quite suddenly. It does not seem to depend upon association elements at all."

We cannot affirm any distinction between "real unconsciousness" and "consciousness without memory." We found that sensory control could be reduced to a minimum both in writing a memorized verse and in taking dictation. Moreover, there were instances where the reagents reported a lapse of awareness covering perhaps several words but they were never confident that at the time of writing there had been no consciousness. As the experiment progressed we came to realize more and more the extent to which a reported lapse of awareness might be a lapse of memory instead. Throughout, there was evidence of progressive restriction of memory, failures to make associative connections, dependent very obviously upon practise. Thus in the dictation experiment breaks occurred, first, in sentence connections, then in phrasal connections,—each word became detached. At the close of even a short interval the reagents were able to recall little of what they had written although confident that at the time of writing they had been aware of it. In the case of D the breaks in connection went further. Meaning evaporated from detached words and sometimes in the middle of a word she forgot what it was.

The progressive failures in memory as the reagents learned with practise to concentrate upon reading; the fact that just about as much could be recalled at the close of a one as at the close of a three minute writing interval in the dictation experiment; the gradual loss of meaning, no doubt through breaks in connection; the more evident failures of memory when the reagents were in bad physical condition, led us to

[16] *Op. cit.*, pp. 501 f.

believe that all lapses reported were memory lapses which approached as a limit a completely dissociated mental bit.

It is significant that A, whose span of attention is broader than D's, should have reported less thoroughgoing breaks than did D, and that he should have remembered so much more of the written contents than D, and at the same time have had also a relatively better memory of what had been read. His success in writing the verses while carrying on other operations seemed also dependent upon his ability to remember "where he was at," it was only toward the close that he began to complain that meaning was evaporating from the verse so that if he lost his place he was unable to continue with the verse unless he thought back over what he had written. D made this complaint much earlier.

In the above discussion we are thinking chiefly of the verbal (cue) consciousness. Movement itself may, it would seem, be a purely physiological occurrence but there is no reason why kinaesthetic sensation from hand and arm may not become dissociated as well as any form of verbal imagery. We have said before that A's writing-habits are so thoroughly mechanized and his writing-impulse so smooth that normally the return report from hand and arm does not enter the focus of attention; it is marginal. D, on the other hand, is acutely conscious of the movements of the hand and arm in writing and reported under distraction a drop in kinaesthetic awareness of the hand that was very distressing. With this went a curious loss of spatial orientation, with, on occasion, some strange illusions as to hand-movement. She found it impossible to maintain alignment, which in the successive writing of the verse became progressively more degenerate.

It would seem that neither of the reagents who served in the experiment was of the narrow span of attention that would seem to favor breaks in associative connections and, hence, loss of conscious integration. A's span of attention is probably exceptional. With subjects of a different attention-type more frequent reports of lapse of awareness might have resulted. In any case material is accumulating as to the probability of striking individual differences in the ease with which consciousness may be "restricted" so to speak. The habit of fainting, the varying susceptibility to intoxication, and of amenability to anaesthetics, the varying ease with which individuals fall asleep point to striking differences in the facility with which breaks in connection occur. It was our impression from the whole course of the experiment that a report of unawareness was certainly conditioned by lack of

memory-connections and that, possibly, consciousness itself should be phrased wholly in terms of connection.[17]

We may meet at this point a few criticisms which may be urged against our method of attempting to get samples of automatic writing. It may be said that the verse tests were such as to demand conscious control, at least to some degree; or that, on the other hand, the use of a memorized content might have led to an illusion of a complete memory since, after the writing was completed, one knew at least what he was supposed to have written. Moreover, the calling for introspective reports on the course of the experiment may have developed an intention to remember and such an intention is without doubt very influential in determining recall. Asking for a report by the reagent of the words written in the dictation experiment may for the same reason be cited as an error in method. But testing of the memory was, of course, necessary in some form. It may be said that our dictation tests were run for too short a period to permit absorption in the story. We had, however, so trained ourselves that we plunged into the reading without effort. Moreover, in the supplementary test we ran the writing for long periods without obtaining any essentially different results. Except with short periods of work it would have been impossible to check the memory reports for, under the conditions of the experiment, memory of what has been done vanishes with very great rapidity. It is the failure to offer any evidence of the checking of general introspective reports that one feels to be a criticism upon the valuable report of Solomons and Stein.

[17] In certain respects this conclusion as to the character of automatic writing may seem at variance with that reached in my monograph on "Control Processes in Modified Handwriting" (Psychol. Rev. Monograph Series, 37). In that report emphasis was placed upon the kind of control utilized in writing under distraction; here, the emphasis is on the functioning of attention; in the former report the initial response to different sorts of distraction was studied; in this report we have worked on the effect of practise on a double operation. The two investigations are supplementary. I have, however, been impressed during the course of the present investigation by the extent to which certain reports of unawareness are certainly conditioned by failure to make connections and by the value of a limiting conception of a detached bit of consciousness. Certainly, in some curious way, the kinaesthetic side represents the crucial point in the situation. Thus D would seem to offer better evidence for the psychophysiological interpretation of automatism than A. The most important difference in the reactions of these reagents concerns the nature and co-ordination of the motor impulse, with, in addition, some evident difference in the natural scope of attention. (J. E. Downey.)

THE STUDY OF DREAMS[1]

A Method Adapted to the Seminary

By Madison Bentley, University of Illinois

The clinical study of dreams has, in our times, attracted a great deal of attention. By many students of the abnormal it is held to be a valuable aid in the diagnosis of mental disorders. The dream, as interpreted by the clinician, is a residue of early emotions and of old desires which find, in sleep, a deferred and vicarious satisfaction. Whatever the diagnostic import of the dream may be, however, it is clear that the psychologist cannot derive from the clinician's methods of divination a coherent descriptive account of the dreaming mind. If dreams are symptoms and symbols, they are also facts of consciousness, facts which present characteristic constellations, which take their proper courses, and which carry functions different in degree, at least, from those of the normal consciousness of the daytime. This clinical method, then, is not well adapted to the study of the dream considered on its own account.

Within a few years, experiment has notably enlarged our knowledge of the dream, particularly of the immediate dependence of the dream upon stimulus and upon bodily state. The studies of de Sanctis,[2] Vold,[3] Weygandt,[4] and others have made it evident, in a striking manner, that motive and incident may be supplied to the dream in a great variety of ways through bodily interference. The application of heat and cold, of lights, sounds, odors, pressures, the constraint and disposition of bodily members, and many other means to stimulation of sense organs, may—as we now know—determine or modify both the main contents and the obscurer context of the dream. Such studies as these afford admirable control of the psycho-

[1] Studies in Psychology from the University of Illinois.

[2] de Sanctis, S., *Die Träume,* Halle, 1901; —— and Neyroz, U., *Psych. Rev.,* ix, 1902, 254.

[3] Vold, J. M., *III. Internat. Congress f. Psychol.* (1896), 1897; *Ueber den Traum,* Leipzig, i, 1910, ii, 1912.

[4] Weygandt, W., *Entstehung der Träume,* (Diss.), Leipzig, 1893; *Philos. Stud.,* xx, 1902, 456; *Zeitschr. f. Psychol. u. Physiol. d. Sinnesorg.,* xxxix, 1905, 1.

physical conditions which obtain during sleep. They have, moreover, led to extremely clever modifications of empirical technique. It should be written down to their credit, too, that these experimental essays set a useful check upon the looser divining methods of interpretation, methods which run the risk of personal bias and of doctrinal contamination.[5]

But neither has the method of abrupt initiation and occasional interference given—any more than the diagnostic method—a delineative account of the dream. To this end careful and complete introspection, taken under the best conditions, seems to be the most direct and the most natural means. Various objections,—it is true,—to this direct method have been urged; the impossibility of immediate report, the abnormal state of the sleeper, subsequent amnesia, and the like. The serious attempt to make methodical record of one's own dreams, for psychological ends, suggests, however, that the objections offered to the method strike at difficulties which are not really insuperable.[6]

But not every dreamer's tale is an introspection. Those recitals of the dream which are made for dramatic purposes, or for entertainment, or as a cathartic means of relieving the oppressed mind, have, it must be admitted, no scientific use; and the dream-story, retailed by the psychologist himself, has,

[5] J. J. Putnam, a believer in the clinical analysis of dreams, speaks of "alleged eccentricities (doubtless sometimes real) in interpretation." *Jour. of Abn. Psych.*, ix, 1914, 36.

[6] Among recent applications of the introspective method to the investigation of dreams should be mentioned the studies of A. Aall (*Zeitschr. f. Psychol.*, lxx, 1914, 125), F. Hacker (*Arch. f. d. ges. Psychol.*, xxi, 1911, 1) and P. Köhler (*ibid.*, xxiii, 1912, 415; xxvii, 1913, 235). Aall prescribes as the proper method of observation (*subjektive Reflexonismethode*) the careful, immediate and unbiased reproduction of the dream by a trained observer, in a quiet dream-like state, with a critical survey of the antecedent waking conditions (p. 126). Hacker, who observed systematically for nearly a year and a half, lays emphasis upon the necessity for (1) immediate transcription and (2) exact and searching analysis and description of the dream-consciousness (pp. 2ff). At the suggestion of Professor Külpe, H. made a special study of thought in dreams. Köhler's observations were based upon about 600 dream-records. He profited by the fact that he is accustomed to waken "twice every night,—rarely three times," by setting down the records during the waking periods of ½-1½ hours. Both K. and H. give curves for depth-of-sleep (Michelson's auditory method). These three studies mark a notable advancement in method over older studies. Our own work differs from them chiefly in its subdivision of the task and in the more elaborate fractionation of the introspections.—The possibilities of combining clinical and analytical motives in the study of the dream are illustrated by the article of E. R. Thompson (*Brit. Jour. of Psych.*, vii, 1914, 300), which was inspired by Freud's *Die Traumdeutung*.

if it is taken as it stands, only a qualified value in description. The recital, though it be given accurately and faithfully, is no more and no less introspective than the account of a ball-game or an enumeration of observed paintings in a gallery. It differs from these only in the extent to which the events detailed are controlled by external or by organic conditions. Like them, it furnishes only gross "information," the raw material from which introspective descriptions have carefully to be separated.

In part, the allurements of the dream-consciousness itself, and in part the possibilities it offers for introspective training led us to choose (in the spring of 1914) the subject of dreams as a topic for the seminary. After historical summaries of the literature, from Aristotle to Freud, had been brought in and discussed, as many members as found the task feasible began the nightly quest for dream materials.

The conditions of the quest were made as follows. In the hand of each observer was placed a copy of the outline reproduced upon the opposite page (Table I). Simplified copies (one for the record of each dream), which contained spaces to be filled in by the observer, were also distributed before the work began. At the same time the Obs. was supplied with candles, matches, pencil, blank paper, an alarm clock, and a sealed schedule of hours of waking made out for the following fortnight. Upon retiring at night the alarm was set, without O's knowledge, according to the schedule; and the necessary materials were placed within reach of his hand. The schedule of hours was so chosen, by a person not an observer, that the waking-signals should ring as if by chance with an equal distribution throughout the period of sleep. Only one arousal was arranged for in any single night. Upon a given evening, then, O knew only that the alarm was to sound at some time before his normal hour for waking. The length and violence of the signal were adjusted by a preliminary practice to the needs of the individual. Thus one Obs. would require only a muffled tap of the bell, while another wakened at the end of a long din released close to his ear. The Obs. was cautioned to begin his record only after he had accustomed himself to a quiet arousal at the alarum. He was instructed to take the introspective attitude (without moving and with eyes still closed)[7] and to wait passively for the resurgence or the *Abklingen* of the dream.[8]

If no dream was at hand, the Obs. recorded the fact, together with the hour, and returned to his slumbers. If a dream appeared, he

[7] P. Köhler, (*Arch. f. ges. Psychol.*, xxiii, 1912, 417) also noticed that the dream tends to disappear under bodily movement.

[8] In 1893 M. W. Calkins made a statistical study of nearly 400 dreams of two Obs. (*Amer. Jour. of Psych.*, v, 1893, 311). In this study the alarm clock was at first used, but soon discarded, the Obs. trusting to accident to rouse him from sleep after dreaming. The method seems to have been suggested to Professor Calkins by J. Nelson (*ibid.*, i, 1888, 381ff.). It is traceable to L. F. A. Maury (*Le sommeil et les rêves*, Paris, 4th ed., 1878, 2ff), who engaged a person to arouse him during the night. For a full discussion of the method see Foucault, M., *Le rêve*, Paris, 1906, 9ff.

was to trace its main features, then to light his candle, set the dream down photographically (sect. I, 1, in Table I), and fill in under the other rubrics as much as could not safely be left to the next day. Upon waking in the morning, the rest of the record was filled out. At our meetings, held fortnightly, the individual records were brought in, together with a summarized report made out upon a blank form which was supplied for the purpose.[9]

TABLE I

Obs.....Date.....Time {Bed..... Asleep..... Aroused.....

General psychophysical condition Remarks

I. 1) Recital: D. events given in order

II. 2) Description: translation into process-terms (including simple feelings and attitudes)

3) Temporal continuity and sequence (or absence)

4) Associative formations {Assimilation; Free images; Discursive A; Topical A}

5) Configuration {Foreground, background; Broad, narrow, deep, shallow; Degrees of clearness; Sustained or fluctuating attention; Primary, secondary or derived-primary attention}

6) Mental functions {Perception, ideation, memory, imagination; Volition, emotion, sentiment, recognition; Thought (or apprehensive, executive and elaborative)}

III. 7) Waking residue of D. {Mental; Bodily}

IV. 8) Persons {Known; Unknown} And Scenes {Recent; Remote}

V. 9) Relevant bodily and external events during sleep (e.g., pain, cramp, muscular tension, fatigue, light, cold, sound)

VI. 10) Reference to past events: waking events, concerns, interests, fears and desires {Evening before; Day before; Earlier}

11) Reference to character, temperament, ideals, principles, individual traits

VII. 12) Gross comparison with waking consciousness {Likenesses; Differences} (e.g., processes, coherence, rate, attention, shift of function, types of association)

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[9] The department is prepared to furnish mimeographed copies of the outline and of the blanks to psychologists who will undertake to collect additional material.

It was not to be expected that each dream would furnish exhaustive materials under each of the twelve headings; instead we hoped to supply, by a kind of fractionation, from one record what was wanting to another. Our chief modifications of the method were (1) the equal and predetermined distribution of arousals throughout the night, (2) the separation of the dream-recital from the introspective description, (3) analysis, by headings, of the introspections, and (4) the giving of instructions which should define and limit the task or *Aufgabe*.[10]

Results.

I. *The Dream and the Sleeping Period.* Since the alarm-signals were distributed with approximate regularity throughout the night, it is possible to make out the relation of dreams to the course of the sleeping period. The signals for waking were arranged to fall on the half-hours, the earliest at 11:30 and the latest at 5:30. The limits were so chosen as to fall within the borders of the customary sleeping period of the observers.[11] If dreams were equally frequent throughout the night and if circumstances for their waking revival were the same from hour to hour, then we should expect to find a like number of dream-records upon each of the signal-hours. Experiments made upon the depth of sleep[12] and dream-records previously published[13] alike lead us, however, to anticipate an unequal distribution. The following Table (II) indicates that most dreams appeared, for our observers, after the second hour of sleep was finished. Only one dream is recorded earlier than two and three-quarters hours after the initiation of sleep. This dream comes from the writer's record and it was noted as a "mere ghost," almost entirely wanting in describable contents. The totals for the whole group of five persons, running from hour to hour, stand as follows:

[10] The essential difference obtaining between the clinical method and the method here proposed will be made evident by a comparison of our Table I with the "dream analysis records" recently published by L. H. Horton (*Jour. of Abn. Psych.*, viii, 1914, 393).

[11] It occasionally happened, of course, that the Obs. was awake at the time of the alarm; but such exceptions were noted.

[12] Michelson, E., (Diss.) Dorpat, 1891, and *Psych. Arbeiten*, ii, 1899, 84 (Michelson cites the work of Kohlschütter, and of Mönninghoff and Piesbergen); Herrwagen, F., *Philos. Stud.*, v, 1889, 301; Sanctis, S. de, and Neyroz, U., *op. cit.*

[13] Calkins, M. W., *op. cit.;* Weed, S. C., and Hallam, F. M., *Amer. Jour. of Psych.*, vii, 1895, 405; Monroe, W. S., *Jour. of Philos. Psych. and S. M.*, ii, 1905, 650; Spiller, G., The Mind of Man, 1902, 430ff.

TABLE II

Hour of Sleep 0–1	0
" " " 1–2	1
" " " 2–3	1
" " " 3–4	4
" " " 4–5	12
" " " 5–6	9
" " " 6–7	12
" " " 7–8	5
Total[14]	44

As regards the small number of dreams recorded for the early hours, there are three possible explanations: (1) The early alarm failed to waken the sleeper, (2) the condition of the Obs. was unfavorable to the discovery of dreams (*e. g.*, on account of the great depth of sleep), and (3) early dreams were actually infrequent. Let us examine the possibilities. Obs. C was aroused only once from 19 alarm signals falling within hours 0-3, and only three times from 25 alarm-signals within hours 3-5. No one of these arousals gave a dream, whereas the 11 arousals in hours 5-7 gave five dreams. Out of 84 signals (Feb. 21-May 22), Obs. A failed to hear only 9, and of these 3, or, possibly 4, fell within the hours 0-3. This Obs. reported a single dream within the first three hours of sleep, out of 13 dreams in all. The other three Obs., for whom data are not quite complete, also returned a number of early dreams disproportionally small for the frequency of arousal. The result accords with unpublished records of the writer, in which a long period of alarm-clock dreams (above 40) gave only blanks or traces for the first two hours of sleep. As regards the second possibility,—loss of early dreams by conditions unfavorable to their recovery,—the records do undoubtedly indicate that the sleeper is apt to be especially stupid and soporose when awakened early in the night. In spite of this fact, however, we were, even while somnolent, clearly aware of the task and we were likewise able to follow the instructions carried over from the evening before. It seems fairly evident, then, that although the early hours of sleep are unfavorable both to arousal and to the introspective attitude, these unfavoring conditions do not fully account for the want of dreams at this time. The absence of dreams is presumably to be referred to the extreme modification of bodily functions incident to deep sleep. It is still arguable, of course, that this extreme modification destroys the incentives to reproduction

[14] Each arousal with a dream is counted as one, although two or more dreams were sometimes reported at the same time.

and produces complete amnesia. The presence of the definite *Aufgabe* to recall makes improbable this assumption, however, which seems, in spite of its frequent support in the literature, to stand in want of positive evidence.[15]

II. *Contents, Course, Configuration and Functions of the Dream* (Sect. II in Table I). We may begin with the processes which make up our 54 dreams. Table III gives the introspective analysis in terms of sensory and affective contents and of attitudes.

TABLE III[16]

Obs.	No. D's.	Visual		Aud'y		Aud'y-Kin.		Kin.	Tact.	Aff'n		Attitude
		Color	Grey	Repr.	Verb.	Repr.	Verb.			Pl.	Unpl.	
A	15	7	15	1	14	..	1	4	3	7	11	16
B	14	1	16	4	14	..	..	6	2	3	8	4
C	9	1	6	..	5	..	..	4	..	1	5	5
D	4	..	4	2	2	..	1	4	..	3	1	7
E	12	2	7	2	7	2	..	1	..	..	2	5
	54	11	48	9	42	2	2	19	5	14	27	37

The frequency of occurrence of sensory and imaginal processes is not especially significant. Our number is too small for statistical treatment.[17] Visual(59) and auditory(51) processes play the chief rôle. Greys(48) are more frequent than colors(11); and verbal auditory(42) processes than tones and noises which are immediately representative of sounding

[15] We leave entirely out of account the period immediately following the initiation of sleep. This period is to be covered by an especial inquiry which will combine experimental control of the hypnagogic state with the present method of arousal and report. Hints at a method are to be found in Ladd, G. T., *Mind*, N. S. i, 1892, 299; Müller, J., *Ueber die phantastischen Gesichtserscheinungen*, 1826, 49; Weygandt, W. E., *Philos. Stud.*, xx, 1902, 464; Vold, J. M., *III. internat. Cong., etc.*, 355; Spiller, G., *op. cit.*, 430.

[16] "Repr" = representative or primary image; "verb" = verbal image; "kin" = kinaesthetic.

[17] Nevertheless the relative frequency of processes from the several sense-modalities stands in substantial agreement with the results of Calkins, M. W., *op. cit.*, 321ff; Weed, S. C., and Hallam, F. M., *op. cit.*, 405ff; Monroe, W. S., *op. cit.*, 650; Hacker, F., *op. cit.*, 16; and Köhler, P., *op. cit.* (1912), 433ff. Thompson, E. R. (*op. cit.*, 305) found among his five subjects a like distribution of imageries in the waking and dreaming consciousnesses. Cf. Beaunis, H., *Amer. Jour. of Psych.*, xiv, 1903, 271.

objects(9). For the most part, the visual materials furnish the scenery of the dream; they provide the perceptual panorama; while verbal processes, in auditory form, carry conversations and give descriptive filling to the events. The infrequency of auditory-kinaesthetic fusions(4) is noticeable. The kinaesthetic elements common to verbal formations in waking life seem to drop out of the dream-words. Kinaesthetic processes, such as were involved in running, opening a door, rubbing objects in the hand, were reported occasionally by all dreamers(19), and in the cases of A and B tactual elements were added. Affection was recorded in 41 cases, unpleasantness about twice as often as pleasantness. The conscious attitudes (*Bewusstseinslagen*) form an important factor in the dream. Most of them bear the aspect of condensed and foreshortened emotions. They are surprise(7), futile worry(5), familiarity(5), hurry(4), perplexity(3), searching anxiety(2), incongruity, foreboding, expectancy, bewilderment, doubt, apology, distaste, dislike, responsibility, confusion and relief,—in all, 37. It is not unlikely that in waking hours many of these attitudes would have been obviously complex, representing emotive adjustments or moods; but as they stood in the dream they appeared to be elemental. It seems likely therefore that this emotive condensation is especially frequent in the dream-state. In a number of cases, however, the attitude is undoubtedly engendered not by the dream-event, but by the instructions. Thus perplexity, futile worry, and searching anxiety were directly traceable,—at least in some dreams,—to the task of reporting. If they were translated into verbal processes they would appear in such forms as "Does my dream really take this course?" "Can I not recover the true ending of this event?" "Have I recovered all the processes that actually ran their course?" The attitude seems to form a part of the tissue of the dream itself, and not to be introduced from the subsequent period of introspection. This sort of attitude is, as we all know, not limited to the dream. Every introspective psychologist recognizes the attitudes of—"responsibility," may we not call them?—which derive from, and, in part, *are,* the instructions lying behind or underneath the observation. In fact, every scientific observation which has not become automatized by practice or habit, involves, I suppose, either the "responsibility" attitude or its expanded and elaborated surrogates.

The course of the dream is commonly described as broken, disjointed, and interrupted; as lacking the continuity imported

into our waking life by perceptual and ideational trains and by sustained interests, occupations, and the like. We found, however, different sorts of dreams, among which we were able to distinguish four types as regards temporal course; fragmentary(11), disjunctive(13) (*i. e.*, abrupt changes and turns, without, however, a loss of integrity), and continuous(19) dreams, and finally, successive dreams carrying a common topic(6).

The account of the *associative formations* in the dream is necessarily gross. We sought only to identify the general types of constellation and sequence. Groups of images were, as might have been expected, most frequent. They carry, in large measure, the scenes and the persons of the dream. Psychologists who hold strictly to the peripheral origin of dreams obviously overlook the fact that while a dream may be initiated or turned by external or bodily events and agencies these events and agencies furnish no adequate explanation of the main part of the dream's contents. To say, for example, that a flash of light turned upon the sleeper's eyelids or a breath of cold air blown across his face produces a dream of celestial or of arctic regions is only to mean that the photic or thermal stimulus releases a set of central functions which themselves incorporate the dream.[18] The traditional account of the dream makes it discursive: it is said to pass abruptly from topic to topic and from scene to irrelevant scene. However, our incomplete survey of associative type reveals both discursive dreams(17) and almost as many(13) topical dreams. The term "topical"[19] does not indeed imply that dream-topics

[18] Even in the hypnagogic or initiaal dream where the organism is still, to some extent, *en rapport* with environmental events it is easy to over-estimate the part played by stimulus. Cf. Ladd's clever explanation of early visual dreams in terms of retinal figures (*op. cit.*). The peripheral or sensory origin of dreams is frequently urged in the literature. The only empirical basis for the theory seems to rest upon the observation that *some* dreams are touched off or turned in their course by stimulus. Hacker (*op. cit.*, 97) credits Purkinje,—as I think wrongly,—with a sensational theory of dreams. Purkinje distinguished sensory, associative, and "complete" dreams. In the last-named "sind alle Seelenvermögen . . . repräsentirt." "Wenn auch der Traum sein sinnliches Material nicht anders woher, als aus dem wachen Leben nehmen kann, so leidet doch dadurch die Freiheit des innern Subjects mit allen seinen geistigen Anlagen keine wesentliche Beschränkung." R. Wagner's *Handwörterbuch der Physiologie*, u. s. w., III.[2] 1846, 452, 456.

[19] See Ebbinghaus, H., *Grundzüge d. Psychol.*, 2nd ed., i, 1905, 694.

were effectively or even sanely treated; but rather that the associative lines ran from persistent topical centres.

The following Table (IV) gives our results for *Configuration and State.* The first three headings indicate the degree of clearness of the dream taken as a whole; the second three the presence of foreground only, of background only, or of both. Wanting a like description of similar total consciousnesses in the waking state, these figures are not very significant. Moreover, the introspections are difficult and they need highly

TABLE IV

Clear	Unclear	Very obscure	Foreground	Background	Fore- and Background
25	9	5	8	15	23

trained observers, for there is danger of confusing memorial fading with primary obscurity. But anyone who is accustomed to introspective work upon attention is able properly to instruct himself for the dream-task. The results may be taken to indicate, at least, that the obscure processes play a greater part in the dream than in waking life. As regards the type of attention, our observers brought in 36 instances of primary and six of secondary or "voluntary" attention. The latter referred to problems of action and of thought.[21]

Wanting a classification or even a catalogue of *mental functions* to serve the purposes of current psychology it was not easy to formulate our instructions under this heading. The seminary contented itself with the use of the gross traditional terms, first coming to an understanding of its own interpretation of them. The topic needs a separate inquiry of its own. The distribution of functions in our reports follows:

TABLE V

Perception	Action	Emotion	Imagination	Aesthetic Sentiments	Thought
48	2	39	1	4	5

Hacker refers the rapid shifts and turns of the dream to the want of determining tendencies, and the bizarre combination of ideas to the fact that the processes tend, as they accumulate in a constellation, to annex subsequent processes which would have fallen, in the waking consciousness, into quite different integrations. On the other hand, Köhler believes that determining tendencies underlay a number of his dreams (*op. cit.,* 1912, 482).

[21] *e. g.,* Obs. A: "Secondary attention occurred when I was listening to the reading of a letter, when I was trying to perform an experiment with rats in spite of numerous distractions, and when I was taking part in an argument."

The term "perception" stands first. Were it not for the frequent confusion among psychologists of perceptual and memorial functions, it would hardly be necessary to explain that "perception" as here used refers neither to the actual presence of objects nor to the adjustment of the organism to its environment. It is a purely mental function and it stands for the apprehension of persons and things, without enquiring whether the persons and things are actually and really *present*. With such an epistemological question the perceiving consciousness has nothing to do. On the other hand, "memory" was taken to mean, not that the previous experience was found by inference to furnish dream materials, but that the dreaming consciousness made direct and explicit reference to a "past" which was distinguished, *within the dream*, from the perceptual or ideational or emotional "present." The term "imagination" is to be taken in the same strict sense. There were no authenticated cases of memory in our dreams,[22] and only one of imagination.[23]

The discrepancy between the number of dreams with perceptions(48) and the total number recorded(54) calls for explanation. To many the dream is just a perceptual panorama. We found several dreams, however, without "scenery" and without events. One was made up of an attitude of expectancy, another was anxious waiting, and a third was "a vague struggle to formulate some such thing as a rule." It seems likely that in dreams of this kind the only conscious processes carried in sleep are those processes which bear the instructions from the preceding day. Such shreds of consciousness may possibly be responsible for the predetermined wakening of persons who "set" themselves for early arousal.

Unlike the attitudes, the *dream-emotions* were seldom referable to instructions. They sprang instead from total situations or scenes, and they include embarrassment(7), fear(7), anger(6), resentment(3), wounded feelings, loneliness, vexation,

[22] Calkins (*op. cit.*, p. 324) reports several cases of dream-memory. However, the context does not make it quite clear that the memorial reference was, in these cases, within the dream-consciousness. The same thing may be said of Köhler's "memories" and "imaginations" which he found in 1/5 of all his dreams. They seem to differ from other ideational processes, not in function, but in attributes; they are, as K. explains, less vivid and corporeal, more indeterminate and fleeting, and less definitely localized (*op. cit.*, 1912, 438).

[23] The only instance was reported by A: "A sort of living Greek frieze. I imagined it and did not really perceive it."

annoyance, mild joy, sorrow, indignation and remorse; nearly all of them unpleasantly toned.[24] Although *action* played a small part in the dream consciousness, as the recitals abundantly testify, it seemed probable to us, at the end, that we had at times overlooked obscure processes concerned with such automatized and habituated actions as walking, grasping objects, and changing bodily position. It remains true, however, that such a thing as an "action-consciousness," a constellation of clear and dark processes preceding and accompanying organic movement was, in our dreams, exceedingly rare. This fact may be connected directly or indirectly with the general motor quiescence of the body.

III. *External and Historical References of the Dream* III(7), IV(8), V(9), VI(10). The immediate residues of the dream were surprisingly scanty. Four times the observers noted a haunting effect left upon the subsequent waking consciousness and nine times bodily disturbances (such as trembling from fear) were referable backward to the dream-state. The psychophysical residues of dreams seem to be much more prominent in those impressive morning dreams which we carry over into the active affairs of the day. Of the many *persons* who appeared in our dreams, we recognized 64 in the recital, while 45 were unknown. Besides, there were frequently supernumerary figures who were massed in the background and observed neither as strangers nor as acquaintances. Of the *scenes*, 14 were familiar and 44 were unfamiliar. The unfamiliar, however, revealed at times glimpses of recognizable places. Of the dream items (persons, places, scenes, events, etc.), 37 were reminiscent of waking occurrences of the previous evening, 20 seemed to have been derived from the day preceding, and 24 from earlier waking experiences. It is probable that a stricter search would have shown recent revivals also of the earlier experiences. The obvious reference to an earlier event tends to make the observer disregard a more re-

[24] This result agrees with Calkins (*op. cit.*, p. 327), whose list of emotions displays a remarkably close resemblance to ours. In 3 of the 4 observers of Weed and Hallam (*op. cit.*, p. 409), unpleasant dreams were much more frequent than pleasant. Weygandt (*Philos. Stud.*, XX, 1902, 488), refers the preponderance of unpleasantness in dreams partly to unfulfilled wishes and partly to such organic stimuli as hunger, thirst, and labored breathing. Most of our emotive unpleasantnesses, however, were connected with ideational constellations.

cent revival which has set the older experience *in Bereitschaft*.[25] The writer has succeeded by persistent effort in tracing all the items of an elaborate dream to the waking consciousness of the preceding day and evening.[26] He inclines to the belief that the primary and chief incentive to reproduction in dreams is furnished by recent cortical functions and that the bizarre and novel character of the dream is owing to the fact that bits are torn from the most diverse interests and topics of the preceding day and composed in a single scene. In spite of this strange conglomeration of items drawn from many sources, however, the dream not infrequently reflects the "personality" of the dreamer; that is to say, his temperament, traits, persistent interests, ethical and social principles, color and temper the dream. Thus A recognizes himself in a tendency to be tardy at appointments, in the wish to work alone, in the dislike of disorder, in sensitiveness to opinion, in contempt for prying persons, in psychological interests, in obstinacy; B finds a fixed habit to economize, a tendency to bashfulness, anxious responsibility for his work, and irritation under criticism. E shrinks from public display, enjoys self-commiseration and is inclined to worry over public engagements. Thus it appears that the dream, although it may be trivial when judged by the day's standards, and although it may be largely occupied with perceptual functions set up by small events of the preceding hours, nevertheless runs in old grooves and is, in part, fashioned by determining tendencies at least as old as the individual dreamer.

Upon the *differences observable between the dream and the waking mind* it is easy to follow the irresponsible judgment of the lay-dreamer and so to generalize. We are told that "people" do not think in dreams, that dreams are wholly irrational, that incongruities are not recognized, that dreams do not pursue a given topic, and what not. In our own intro-

[25] *E. g.*, the dream-death of an acquaintance of earlier years due to a funeral procession witnessed the day before the dream occurred. Cf. Spiller, G. (*op. cit.*, 449-450) who cites other instances. The veiled connection of the dream with the preceding day is noted also by Hacker (*op. cit.*, p. 113), especially in early dreams. Light morning dreams more frequently forecast the usual day's routine.

[26] It is natural to suppose that the frequency of dreams of nakedness, exposure, shame, and the like, is due to the preceding preparation for bed. Private and domestic scenes woven into public appearances and social encounters do not fail to offend the dreamer's sense of the proprieties. Perhaps many of the sexual motives cherished by the Freudian proceed from this innocent cause.

spections we found not one of these generalizations to hold.[27] Besides the observations made upon each individual dream-record, the observers attempted, at the end of the week, to draw wider distinctions; but most of these distinctions were either qualified by the individual observer or by a comparison of all the reports when thrown together. Whatever is left over for general formulation may be put in the following words. The waking functions least disturbed are the perceptual functions. The chief disturbance to them is referable to breaks in perceptual trains which are provided against, in waking, by the continuity of environmental changes. The sensory materials of perception are, also, as we have seen, more limited and less various than those provided by the active and alert organism. It is strange that the perceptual functions should be so well sustained under circumstances which make it impossible for the organism,—out of touch with its surroundings during sleep,—to profit by them. The disturbance to the central nervous activities seems, however, less to derange those unknown processes which preserve bits of antecedent function than to destroy the synergetic operations of the cerebrum;—the operations which are necessary to balance and control, and which integrate at once racial and habituated sets, determining and associative tendencies, and the immediate effects of external and organic stimuli. The result for consciousness is that thinking is rare and, as a rule, ineffective; that secondary attention is fitful and incidental; and that those wide integrations which are compassed by the human psychophysical organism in its most productive moments are almost entirely wanting.

Our introspective results are indicative rather than constructive. Even when added to the whole mass of antecedent work upon the subject they do not wholly suffice for the foundation of a doctrine of the dream-consciousness. But the method does, when it is taken seriously in hand by the psychologist, yield results. The writer's experience with it leads him to believe that a person thoroughly trained in introspection can, with practice and with definite instructions, reproduce the dream-consciousness with almost photographic fidelity. At the very least, the method is capable of carrying our knowledge a long way beyond the limits of loose generalization. Moreover, it

[27] Calkins (1893) came to a like conclusion. The danger of drawing wide inferences from the dreams of a single Obs. appears in S. Meyer (*Zeitschr. f. Psychol.*, liii, 1909, 206), who argues on the basis of his introspections against the hallucinatory character of the dream.

may, by forming an alliance with the external agencies of bodily control, be carried to a much higher state of specialization than we have yet attempted.

As regards the second, the pedagogical aim of the inquiry, the members of the seminary were quite agreed at the conclusion of the task that the method admirably served our purpose. It compelled us to use psychological terms with a common import, it defined and quickened our powers of observation, and it convinced us that the dream-consciousness, cut off as it is from the confused interests and demands of our daylight lives, is adapted in a unique and special way to introspective examination.

TESTS ON ADAPTIVE INTELLIGENCE IN DOGS AND CATS, AS COMPARED WITH ADAPTIVE INTELLIGENCE IN RHESUS MONKEYS

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This paper is a report of experiments which were made with dogs and with cats to ascertain to what degree they compare in adaptive intelligence with Rhesus monkeys. The tests on the latter animals, which have been already reported,[1] were made the basis of the experiments on the dogs and cats and will be restated here. Following that, we shall give an outline of the results of similar tests made on the other animals.

By the term "adaptive intelligence," we may designate a lower variety of reasoning. The term reasoning has been used in a variety of ways, and the following definitions include the most important meanings that have been given the term: *A*, implied reasoning (Harris), e. g., my recognition of yonder horse; *B*, inference from particular to particular (James), e. g., the bird which finds bread upon the window one morning comes back the next morning; *C*, adaptive intelligence, the ability to adapt to our purposes conditions more or less difficult and more or less unfamiliar; *D*, analogical reasoning, which involves construction or creation, e. g., to reach an upper window, I utilize a ladder which I find; *E*, rational thinking (James); *F*, formal or syllogistic reasoning.

With the purpose of investigating the matter of the presence of "adaptive intelligence," as above defined, in Rhesus monkeys, the following experiments were made on those animals:

Adaptive Intelligence in Monkeys.—1. A piece of twine was let hang in front of and twelve inches away from the cage, beyond the reach of the longest-armed animal which was tested. At the end of the twine a piece of banana was arranged; a thin piece of wood was pushed through the banana and turned so that one end could be grasped by an

[1] W. T. Shepherd, Some Mental Processes of the Monkeys, *Psychol. Rev. Mon. Sup.*, XII, 1911.

animal in the cage. By grasping and pulling the stick inwards, the food could be secured. All the eleven animals were tested in this experiment. The results with all but one of the animals were similar, and a description of the actions of the one will suffice to indicate the reactions of all. As soon as the banana and the stick were arranged, monkey 6 put her arm through the wire of the cage, seized the end of the stick, drew it towards her, and secured the banana. This experiment was repeated a number of times and in all there was an immediate characteristic response. There appeared to be a decided adaption of means to end. No efforts were wasted on random movements. It did not appear that any preliminary attempt was made to grasp or even to reach for the food, but there was an immediate movement toward the stick. The results for all animals are given in Table I, in which are shown the approximate time for the performance of the act by each animal in each trial. The absence of hesitation, the direction of the movement away from the food towards the stick, and the promptness with which the food was seized, all speak for the presence of adaptive intelligence in ten of the monkeys. It would seem that it is always found in these animals.

TABLE I

ADAPTIVE INTELLIGENCE. *Suspended food and stick. Three or six trials each animal. Times in seconds; f means failed*

Trials and animals	1	2	3	4	5	6
1	60	10	3			
2	4	3	3			
3	20	6	9			
4	105	40	11			
5	f	f	f			
6	2	1	1			
7	6	4	3	1	1	1
8	6	4	4	3	3	3
9	5	3	3	3	4	2
10	2	1	1	1	1	1
11	5	5	3	3	3	3

In this test the results with monkey 5 were decidedly different from those with the other animals, in that he failed in the trials given him. Monkeys 1 and 4 had considerable difficulty in getting the food in the first and second tests;

but there was no gradual acquisition of the method of securing food in the other eight animals. The times for solving the problem in the second and third tests were approximately the same as those in the first test for monkeys 3, 6, and 10; and there was not much difference in the time between the first and later tests for monkeys 3, 7, 8, 9, and 11. Much if not all the difference in time can be accounted for by the better adjustment to the wire netting of the cage, the pushing of the hand through the proper place, etc.

II. A second test of the presence of adaptive intelligence was made as follows: A light wooden lever 18.5 inches long was attached by leather hinges at one end to a board which rested on the floor. The hinge of the lever was four inches from the end of the horizontal board, well within reach of the animals. The lever was inclined at an angle of approximately 45 degrees from the horizontal, and could be moved forward in a vertical plane. The apparatus was placed outside a cage, the lower end of the lever being within, the upper end being beyond the reach of the animal. A piece of banana was placed at the farther end of the lever, and the problem for the animal to solve was how to secure the food, which was beyond direct reach.

Eleven monkeys were tested with this apparatus, and all with the exception of monkey 4 succeeded in the first trial. In the first tests the animals usually took a longer time to get the food; but as in the previous experiment this delay was largely one of making the adjustment of the hand to the proper opening in the wire netting and not to attacking the apparatus in the wrong manner.

Monkey 4 appeared to be frightened at the time of the first trial and I showed him that food was to be secured by moving the lever against the cage and permitting him to secure the food. In the succeeding trials he immediately attacked the lever and obtained the food in the same manner as the other animals. Monkey 9 was in the cage with Monkey 8 when the latter was being tested and may have taken the opportunity to observe Monkey 8. That she performed the trick may have been due to imitation; but I credit her with having performed it in the same way as the other animals. It is reasonable to suppose that Monkey 4 would have been able to manipulate the apparatus without being shown if sufficient time had been allowed. In many of the experiments the animals did not use both hands for moving the lever, but one hand for moving the lever and the other

for securing the food when the upper part of the lever was within reach. Table 2 gives the time records for four monkeys in this experiment; those for the other animals were approximately the same as those noted in the table, although no accurate measurement by watch was taken.

III. One end of a piece of twine, one yard long, was attached to a bucket and the other end was placed within the cage. The bucket was placed at the distance of the length of the twine from the cage; and a piece of food was fastened to the top of the bucket in sight of the animal. The problem was for the animal to draw in the bucket by pulling on the string and thus secure the food. In all cases, the animals seized the twine immediately, drew the bucket toward the cage and seized the piece of banana.

Table III gives the times for Monkeys 7 and 8. These times are similar to those of the other animals.

TABLE II

Trials and animals	1	2	3
1	9	4	3
2	6	4	3
3	2/5	2	2
4	f	6	3

TABLE III

Trials and animals	1	2	3	4	5	6	7	8	9	10
7	6	4	3	3	4	3	3	4	3	3
8	0	3	3	2	2	3	2	2	2	3

The results of the foregoing experiments indicate the possession by monkeys of adaptive intelligence, of a low form of reasoning. The times for the performance of the different acts are reasonably conclusive, but in addition, the appearance of the animals, their actions, etc., especially during the first trial in each experiment, seemed to show an understanding of the problem. There was no fumbling with the apparatus, no appearance of learning by trial and error; but there was instant action following apparently, instantaneous understanding of the situation.

Adaptive Intelligence in Dogs.—As a test of adaptive intelligence in dogs, as compared with that shown by the monkeys in the preceding experiments, I repeated some of the tests made with the monkeys. Three individuals were thus tested. Dog 1 was a rather small mongrel male, two and a half years old; Dog 2 was a large male, part hound, four years old;

Dog 3 was a female, medium size, of uncertain stock, three years old. The results with all the animals were negative, and will be reported briefly.

I. The first experiment was similar to the first test made with the monkeys. Food was suspended by a cord in front and just beyond reach of the animal. A stick was passed through the food and one end extended a short distance within the cage, so the dog might seize the end of it with his teeth. By thus seizing the end of the stick within the cage, the animal could draw the food up and within the cage and so secure the piece of meat. It will be seen that the dogs had a slight advantage of the monkeys by this arrangement.[2]

The results, however, were wholly negative. The dogs scrambled about the side of the cage next to the suspended meat, pawed and sometimes bit at the side of the cage, but made no effective attempt to seize the end of the stick and draw in the food. They each appeared to have no understanding whatever of the problem. They all failed entirely in six trials of one minute each.

II. A second experiment similar to the third experiment with the monkeys was made, i. e., a bucket was placed at the distance of a string one yard long, and with a piece of meat fastened to the top, and in sight of the dog, having the loose end of the string within the cage. The dog, if he possessed the adaptive intelligence, could seize the end of the string in his teeth, draw up the bucket to the cage and thereby secure the food.

In this test again, unlike the monkeys, the dogs all failed, apparently showing no understanding of the problem and no ability to cope with it. There was only a general scramble around the cage, and at the side next the bucket, at the sight of the tempting meat. Sometimes they would appear to give it up.

Here again we appear to see the superiority of the monkey intelligence in dealing with difficulties and in adapting conditions to their needs. We must, however, remember the superiority of the monkeys' motor equipment for dealing with this sort of problem. It might not be safe to conclude that a dog could not do equally as well on his own ground, e. g., in a hunting problem. Furthermore, I do not doubt but that, as Thorndike[3] has pointed out, the apparent superior general

[2] It should be remembered that in a similar experiment with the monkeys the nearest end of the stick was outside the cage.

[3] E. L. Thorndike, The Mental Life of the Monkeys, *Psychol. Rev. Mon. Sup.* III, 1901.

intelligence of the primates is largely accounted for by their superior sensori-motor equipment. But I still believe that they possess a modicum of something resembling real mentality, not present in other animals. On an evolutionary hypothesis, we might expect such to be the case.

Adaptive Intelligence in Cats.—I. Two cats were used in this experiment. Both were full grown and apparently of fair intelligence, although neither was of very active habits.

Both cats were tested under experimental conditions similar to those in experiments with both monkeys and dogs, i. e., a piece of meat was suspended in front of the cat's cage, with a stick run through it, and as in the case of the dogs, projected through the wire into the cage.

In brief, the results were similar to those with the dogs: There was a total failure to understand the problem, or even to try to seize the stick and pull in the food. The cats scrambled about the cage at the side next the suspended meat, but it never seemed to occur to them to use the stick as a means of drawing in the food. Each animal was given six trials, and two minutes were allowed in each trial.

II. The second test was similar to the second experiment with the monkeys,—that is, the inclined lever with food on the outer end was employed. The cat was to reach its paw through the wire, seize the lever end which was within its reach and thus draw up the food to the cage. The results were similar to the results of the previous experiment. There was an entire absence of any evidence of an adaptive intelligence similar to that shown by the monkeys in a like experiment.

We conclude, therefore, that both dogs and cats are absolutely inferior to Rhesus monkeys in dealing with problems of the character of those herein reported and with experimental conditions of a similar nature. This, however, may be due in part to the superior motor equipment of the primates. It is also possible that under experimental conditions more favorable to them, dogs and cats might show an equal or even a superior ability to that of monkeys. However, the writer ventures to infer that in the matter of adaptive intelligence in general, Rhesus monkeys are much superior to their humbler congeners.

EXPERIMENTAL STUDIES IN RECALL AND RECOGNITION

By Edith F. Mulhall, Barnard College, Columbia University

The greater part of experimental work on memory has been on recall. More recently investigators have employed the process of recognition, but few of the differences between the two processes have been reported. The aim of the following experiments was to study the differences between recall and recognition. Tests were made to measure (1) the equivalence of repetitions for recall and recognition, (2) the influence of determination to remember, and (3) the effect of primacy and recency on both recall and recognition.

The Equivalence of Repetitions for Recall and Recognition.[1]

The difference in the number of repetitions necessary for the mastery of fifteen items was determined for four materials, pictures of objects (lamp, fish, clock, etc.), geometrical forms, words (chair, hammer, car, doll, etc.), and nonsense syllables (fik, vod, deb, ruz, biv, etc.).

Each subject was shown fifteen words successively at regular intervals of two seconds each and then required to write those he remembered. A set of thirty (30) containing the original fifteen (15) was given to the subject from which he was to select fifteen which he thought were in the original set. The first set of fifteen was presented again as before and the subject was requested to recall those he could and then to select fifteen from the thirty set. This was repeated until he was able to recall and recognize the fifteen items correctly. The same experiment was performed for all four materials on twenty-five subjects, all undergraduate or graduate students in Columbia University.

Table I shows the average number of repetitions necessary for complete recall and complete recognition, and the ratio of the two for all materials.

[1] Mulhall. Equivalence of Repetitions for Recall and Recognition, J. of Phil., vol. XI, No. 15, p. 411.

TABLE I

Material	*Process*	*Av. No. Repetitions*	*M. V.*	*Ratio Av. for Recall to Av. for Recog.*
Pictures	Recall	3.36	.79	3.23
	Recog	1.04	.08	
Forms	Recall	3.96	.85	2.2
	Recog	1.80	.81	
Words	Recall	4.76	1.61	1.80
	Recog	2.64	.96	
Syllables	Recall	7.12	2.23	1.22
	Recog	5.80	2.11	

The data indicate that the difference between recall and recognition memory is greatest for pictures, somewhat less for forms and words, and least for nonsense syllables. The number of repetitions required for both recall and recognition is least for pictures, greater for forms and words, and greatest for syllables. In examining the materials one finds that the pictures offer the greatest richness of associations. The forms offer somewhat fewer advantages—the shape can be visualized, in a few cases they can be named, and associations, but few in number, can be made. The third material, words, all being nouns, are names and have associations, but lack any particular picture or form element. The syllables, as their name implies, are nonsense; most, if not all, are devoid of any association. Introspections of the subjects indicate that association was employed as an aid in memorizing.

The following secondary experiment shows this more definitely. To each of two subjects a mixed set of sixteen items (four pictures, four forms, four words, four syllables) was presented, one unit every two seconds. Careful introspections were called for as each item was afterwards recalled. The subjects were also required to select sixteen from a set of thirty-two (eight of each material). A few of the records are given as examples of the means the subjects used to aid memory.

Pictures remembered	*Why and How*
butterfly	Benefit Performance of Madam Butterfly to be given for the College.
car	Its many uses, with engine, auto, etc.
coat	Livery.
bridge	A particular bridge in Sullivan County.
lamp	Toaster received yesterday which I connected to a lamp.
nuts	Walnuts I bought today.
Words	
tree	Curious tree examined on a recent walk.
knife	Bread.
Syllables	
naf	Nap.
jod	Jodl, psychologist.

Forms: The forms reminded the observers of "a diamond," "a windmill," "exclamation point," "color disc," "pie," and "kindergarten shapes."

The introspections of both subjects show clearly the method of association in recalling the material and the difficulty and often inability of remembering material with few or no associations.

Since the greatest difference in recall and recognition memory is for pictures when the material has an abundance of association, less for forms and words, and least for syllables where associations are respectively less, it appears that the difference in recall and recognition memory is in part dependent on the richness of associations present.

The Influence of Determination to Remember on Recall and Recognition

The influence of determination to remember on recall and recognition was determined for two materials, one rich with associations, the other devoid of them. Twenty (20) photographs (15 men, 5 women) with names attached, and fifteen (15) nonsense syllables were used. Fifty subjects were tested, twenty-five with no determination to remember and twenty-five with determination to remember the material. The first

group was asked to take part in an experiment in judgment. Each subject read the following instructions:

> "Arrange the 15 nonsense syllables in four piles according to ease of articulation. You will be given two minutes to do this."

and was given a set of fifteen (15) cards on each of which was typewritten one syllable, and four cards labeling the four piles, "Very Easy," "Fairly Easy," "Slightly Easy," "Indifferent." If the arrangement was made before the two minutes had expired the subject was urged to make sure the arrangement was satisfactory. At the end of the two minutes the observer was asked to make a record of the syllables in each pile on the blank provided.

Twenty photographs with names attached and four cards labeling the piles "Very Attractive," "Fairly Attractive," "Slightly Attractive," "Indifferent," were given to each observer with the following instructions:

> "Arrange the 20 pictures in four piles as indicated. You will be given 2 minutes to do this."

After the two minutes had expired each subject was requested to record the names of the pictures in each pile on the blank provided. This was done to make sure the subject looked at the names under the pictures.

The subject's memory for both materials was tested. Three minutes were given for the recall of the syllables and then a selection of fifteen from a set of thirty, fifteen of which were in the original set, was made. The observer was asked to recall the names of photographs in three minutes. A set of forty photographs, twenty being those of the original set without names, was provided with the following directions:

> "Select 20 pictures from this group which you think were previously shown. Name any which you can."

After the subject had selected the photographs, a set of forty cards on each of which was typewritten a name (twenty were the names attached to the photographs in the original set, twenty were other names) was used for the recognition of names. Care was taken not to repeat any name, either first or last, nor to use the last name of any well-known person.

The second group of twenty-five subjects first copied the list of syllables and names on the pictures. This was done in order to control the conditions, keeping them, as far as possible, identical with those of the first group. If such a record

had not been required for the group which was judging, one could not be sure that the subjects read the names. Even with this precaution, the subjects claimed they had never looked at them, until reminded they had written each on the blank.

After making these copies, the subjects of the second group were given the set of fifteen nonsense syllables with these instructions:

> "You will be given 2 minutes to look at these 15 syllables. Later you will be called upon to remember them, so look at each with a *determination to remember it.*"

After the two minutes had expired, the set of photographs was presented with the directions:

> "You will be given 2 minutes to look at these 20 pictures. You will afterwards be called upon to remember the names and photographs, so look at each with a *determination to remember.*"

Recall and recognition were tested in the same way as for the first group.

Throughout the experiment an attempt was made to keep the conditions for both groups the same. Any sources of error or awkwardness in conducting the experiment, due to the difficulty of keeping the observers of the first group ignorant of the purpose of the investigation, which may give rise to criticism, will be found to be constant for both groups.

The purpose of the experiment was (1) to compare recall memory, with and without determination to remember, with recognition, with and without determination to remember, respectively; (2) to compare recall and recognition with determination and recall and recognition with no factor of determination present; and (3) (a) to compare recall of syllables, with and without the intention to remember, with recall of names with and without determination, and (b) to compare recognition of syllables with and without the intention to remember with recognition of photographs and names with and without the intention to remember.

In Table II the total number recalled and recognized by both groups is given for both materials. Table III shows the ratio of recall with a determining factor present to recall with no such factor present and the ratio of recognition with a determining factor to recognition without the determining tendency for both materials. Table IV gives the ratio of recog-

nition with determination to recall with determination and the ratio of recognition without determination to recall without determination for both materials.

TABLE II

SYLLABLES

	No Determination		Determination	
	Recall	*Recog.*	*Recall*	*Recog.*
Total	126	318	155	326
Average	5.0	12.7	6.20	13.0
M. V	1.65	1.26	2.06	.73

PHOTOGRAPHS AND NAMES

	No Determination			Determination		
	Recall	*Recog. N.*	*Recog. Ph.*	*Recall*	*Recog. N.*	*Recog. Ph.*
Total	77	391	461	134.5	435	415
Average	3.08	15.6	18.4	5.36	17.4	16.6
M. V	.90	1.53	1.48	1.57	1.44	1.90

TABLE III

SYLLABLES

Ratio of Determined Recall to Undetermined Recall	100 : 81.3
Ratio of Determined Recog. to Undetermined Recog	100 : 97.5

PHOTOGRAPHS AND NAMES

Ratio of Determined Recall to Undetermined Recall	100 : 57.2
Ratio of Determined Recog. to Undetermined Recog	100 : 99.7

TABLE IV

SYLLABLES

Ratio of Determined Recog. to Determined Recall	100 : 47.55
Ratio of Undetermined Recog. to Undetermined Recall	100 : 39.62

PHOTOGRAPHS AND NAMES

Ratio of Determined Recog. to Determined Recall	100 : 15.82
Ratio of Undetermined Recog. to Undetermined Recall	100 : 9.03

TABLE V

Ratio of Det. Rc. of SYLLABLES to Det. Rc. of NAMES	100 : 86.8
Ratio of Undet. Rc. of SYLLABLES to Undet. Rc. of NAMES	100 : 61.1
Ratio of Det. Rg. of SYLLABLES to Det. Rg. of NAMES and PHOTOGRAPHS	100 : 260.6
Ratio of Undet. Rg. of SYLLABLES to Undet. Rg. of NAMES and PHOTOGRAPHS	100 : 267.9

In Table V the materials are compared. The ratios of recall of syllables to recall of names are presented with no determination and with determination to remember; the ratios of recognition of syllables to recognition of photographs and names with and without determination to remember are also stated.

It will be noticed that fewer photographs were recognized when there was a determination to remember present, but many more names. This does not mean that determination to remember decreases one's ability to recognize faces. The larger number of names recognized with determination to remember seems to indicate that the observers considered names more difficult to remember and spent most of the time allotted to them to learn names. The total number of photographs and names recognized with determination to remember was 850 and without 852, or an average in each case of 34 items. Throughout, the calculations involving the recognition of the material with greater meaning the totals 850 and 852 have been used. Frequently a subject could recall the first or last name but not both; in each case the score of one-half was given (Table II).

TABLE VI

RECALL OF NAMES

	No Determination			Determination		
	1st only	2nd only	Both	1st only	2nd only	Both
Total	40	50	32	49	62	79
Total First Names		72			128	
Total Last Names		82			141	

The data indicate the following: (1) the factor of determination to remember influences recall memory, but its effect on recognition is little, if any; (2) the difference between recall and recognition is less when there is a determination to remember the material than when there is no intention to remember; (3) the influence of determination for the recall of names is greater than for the recall of nonsense syllables; there is little, if any effect of determination to remember on the total number of items recognized of either material.

A further study of the data for the recall of names shows interesting results. When no determining factor was present 72 first names were recalled and with determination 128, or an increase of 77 per cent; with no determination to remember 82 last names were recalled, with intention present 141, or an

increase of 72 per cent; with no determination total number recalled 77 and with determination 134.5, or an increase of 75 per cent. Determination to remember seems to influence the recall of first and last names to the same degree. Determination to remember influences greatly the number of first and last names correctly connected, as there were only 32 with no determining factor and 79 with one, or an increase of 147 per cent. Moreover, the determining tendency influences the number of photographs which may be correctly named. Table

TABLE VII

NAMES CORRECTLY ASSOCIATED WITH PHOTOGRAPHS

	No Determination				Determination			
	1st	2nd	Both	Total	1st	2nd	Both	Total
Total.....	2	9	13	18.5	10	24	48	65.0
Highest Score...	1	3	4	4	3	4	7	8
Lowest Score....	0	0	0	0	0	0	0	0

VII shows the number of first, last, and whole names which were correctly assigned to the photographs for both groups of subjects. According to the method of scoring adopted, 18.5 names were correctly given to the photographs by the first group and 65 by the second.

The Effect of Primacy and Recency

The third part of the investigation was concerned with determining the influence of primacy and recency on recall and recognition. Thirty-eight subjects were tested for two materials, nonsense syllables and photographs. To nineteen, a list of twenty-four nonsense syllables was presented at the rate of one item every two seconds in a given order 1 through 24. Immediately after the presentation they were asked to recall all they could in three minutes and then were given a list of forty-eight from which to select 24 which they thought had been previously presented. The subjects were then shown twenty-four pictures at the rate of one every two seconds. As each was presented a name was pronounced by the experimenter. Immediately after the presentation, they were given three minutes in which to recall the names. A set of forty-eight photographs, containing the original twenty-four, was used for the recognition test. Each subject was then provided with a list of forty-eight names from which to select twenty-four which he thought had been pronounced as the pictures were presented. The experiment was repeated on

the second group of nineteen, but the photographs were shown first and the syllables second, and the order of each material was 13 through 24, 1 through 12.

The results were calculated according to the position of the item, *i. e.*, for position 1 the record is given for the number of times item 1 was remembered by members of the first group, and item 13 by members of the second group; position 2, the number of times item 2 was remembered by the first group, and item 14 by the second group; and so on until position 24 shows the number of times item 24 and item 12 were remembered by the first and second groups respectively. (Table VIII.)

TABLE VIII (see Charts)

PRIMACY AND RECENCY

	Syllables		Photographs and Names		
Position	*Recall*	*Recog.*	*Recall* N.	*Recog.* N.	*Recog. Ph.*
1	23	36	3.5	31	27
2	16	34	0	14	19
3	19	34	5	34	28
4	14	27	3	29	17
5	9	32	3.5	29	20
6	10	19	4	29	22
7	6	30	2.5	30	20
8	2	28	4.5	24	32
9	10	29	2.5	24	20
10	8	26	3.5	21	16
11	8	28	4.5	24	27
12	6	27	2.0	24	17
13	11	30	7.5	26	28
14	11	26	.5	19	15
15	9	19	9.0	31	26
16	10	23	4	23	12
17	6	28	4	22	25
18	5	20	3 5	22	25
19	10	27	2	21	24
20	10	20	6.5	29	29
21	6	22	5	20	15
22	19	32	11	24	18
23	11	22	20	35	32
24	21	31	30.5	33	13
Average	10	27	5.9	25.7	21.9

Since one is able to recognize much more than one can recall, any factor which increases recall memory by one unit may not have the same influence on recognition memory when it, too, is increased by one item. The relation of the total number recalled for each position to the average number recalled of all the positions is used as a measure of the effect of position for recall and the relation of the total number

recognized for each position to the average number recognized is used as the measure of the effect of position for recognition. The accompanying charts show the influence of primacy and recency for both processes and for both materials.

Primacy and recency influence both recall and recognition of nonsense syllables, both are more effective in the former than in the latter case. As far as the data of this experiment indicate, the influence of primacy and recency is about the same. Turning to the chart for names and photographs, one notices no effect of either primacy or recency for recognition. Primacy has only a slight effect, if any, for recall, but recency shows a marked influence on recall.

The results will allow only suggestive conclusions, but it

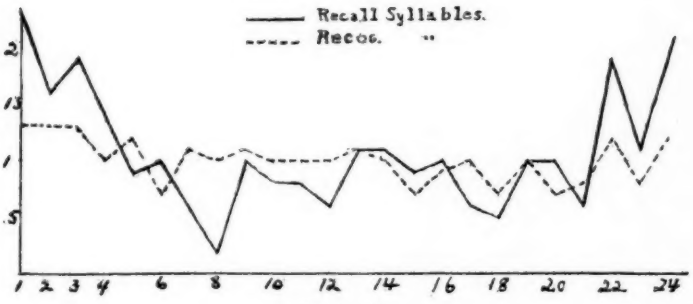

CHART I

Horizontal Scale: Position of the item. Vertical Scale: Ratio of the number remembered for each position to the average.

appears that primacy and recency influence recall memory more than recognition. The influence of both on recognition is greater the less meaning the material has.

The results of experimental studies in memory may lead to practical applications. We have long known that the value of a single presentation is greater in recognition than in recall, and we may add that the difference in the value of repetitions is greater for material with meaning and less for material without meaning. Advertisers will find it takes fewer presentations for prospective customers to recall and recognize a trade-mark which has a wealth of associations.

Banks are eager for competent detectives who will recognize and often associate the correct name to each man and woman who is a depositor. The individual differences shown in

Table VII indicate that people vary in their ability to identify and name faces. A test similar to this might serve as a means of selecting competent bank detectives.

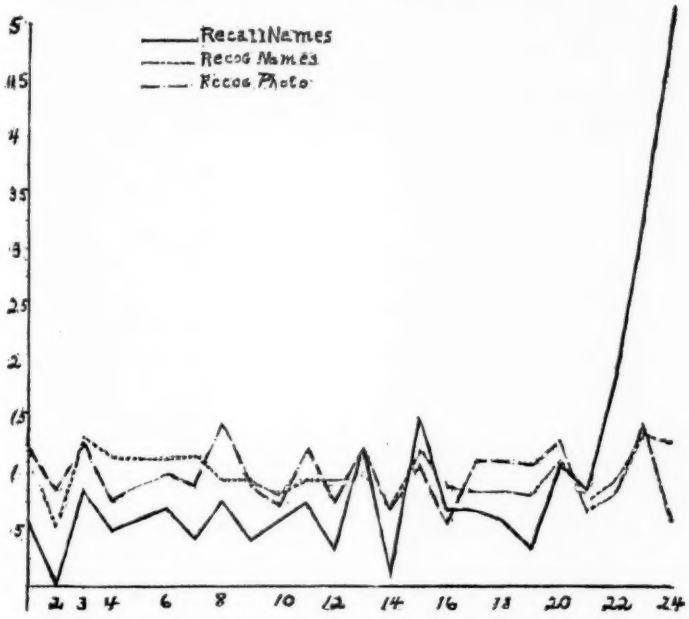

CHART II

Summary

In general, the difference between recall and recognition varies with the materials remembered and with the same material when different influences are present.

1. The difference between recall and recognition is greatest when the material is rich with associations and becomes less the fewer the associations in the material.

2. Determined recall differs from undetermined recall more than determined recognition differs from undetermined recognition.

3. The difference between determined recall and determined recognition is less than that between undetermined recall and undetermined recognition.

4. The influence of a determining factor is greater for recall of material rich with associations than for material devoid of them.

5. The determining factor influences the amount of material remembered which can be correctly associated with other material remembered.

6. Primacy and recency both influence recall memory. The influence of each on recognition is less than on recall, but is greater for material devoid of associations and less for material rich with associations.

NOTE ON THE PSYCHOLOGY OF SHAME[1]

By THEODATE L. SMITH

In the field of emotion experimental psychology has as yet offered us but little, for only the simplest emotions have proved available for laboratory investigation, and the results of plethysmographic experiments have thus far not been commensurate with the amount of labor bestowed upon them and are lacking in definiteness. In the present study, while introspection has, of course, been the final test, the material has been drawn from anthropology, animal psychology, and the study of children and defectives. Literature and biography have also furnished illustrative material. I have found but one monograph on the subject, that of Hohenemser, which is written entirely from the introspective point of view, and his four possible types of shame are more strongly suggestive of logical than psychological possibilities and apply only to a highly developed self-consciousness.

Most psychologists seems to have overlooked the existence of shame as a separate emotion, or, at most, devoted a line or two to it in connection with allied mixed emotions. Ribot places modesty, shame, and shyness in a group of emotions which he characterizes as "based on an association of intellectual states, which is, in most cases, an association by contrast," and "presupposes a fusion, in varying proportions, of agreeable and disagreeable states." He further adds that the emotion, as a whole, differs from the sum of its constituent elements, which analysis can describe and isolate. Bain resolves shame into a dread of being condemned or ill-thought of by others. ("Emotions and Will," p. 211.) James finds the origin of the emotional states of modesty, shame, and shyness in the application to ourselves of a judgment previously formed upon others. Baldwin defines shame as a "lowered self esteem, felt with reference to something positive pertaining to self and open to the knowledge or opinion of others."

The motives most closely allied to shame and frequently confused with it are shyness, bashfulness, embarrassment, mortification, humiliation, coyness, and modesty; indeed the

[1] An unfinished paper found among the author's manuscripts after her death on February 16, 1914.

German has but one term for shame and modesty, yet modesty, whether we take it in its broader definition of "the form of timidity or shyness due to reflective self-consciousness," or the narrow anthropological definition of "physical self-respect," is not in itself a distinctively painful emotion, since unpleasantness arises only in connection with any violation of it. The confusion is indeed due to anthropology rather than to psychology, and is connected with the theory advanced by some anthropologists and by Havelock Ellis that modesty and shame have their origin in the sex instinct.

To the writer, shame introspectively considered seems to consist of a special case of what Lipps has described as *psychische Stauung,* a sort of paralysis which occurs whenever two or more tendencies, each of which tends to appropriate the psychic powers, are active, thus producing, at the point of inhibition, an accumulation of psychic force. There is always present in shame a disharmony in the content of consciousness, a feeling that the social self has lost value. It is not always fear of the opinion of others, for it is possible to be ashamed before one's self, when there is no question of the knowledge of others. This psychic paralysis is paralleled in the physical manifestations of shame, which are muscular weakness, sinking of the head, drooping of the eyes and whole body, accompanied by vaso-motor reactions, which oftenest take the form of flushing, but are sometimes manifested by pallor. In little children the tendency to hide, also manifested in fear, is apparent.

If this analysis is correct, then the capability of experiencing shame must be directly dependent upon the development of the social self-consciousness. In animals, we should expect to find it manifested only among gregarious types or those more or less domesticated and dependent upon human care and companionship; in children, as a somewhat later development than the more primitive emotions of fear and anger—and in defectives in proportion to the degree to which they are capable of becoming socialized. Is this in accordance with the facts? Observations on 814 cases of shame in children from 2 to 15 years and 402 reminiscent cases in adolescents were collected and analyzed. No cases of shame were reported in children under two years, though Preyer mentions one very doubtful example in his son at 10 months. His first positive record, however, is at 26 months. The occasions of shame in young children are very clearly reflections of their environment, typical examples being muddy clothes, dirty face, being seen with a nursing bottle after

having learned to eat, a hole in the stocking, sores on the face, being scolded in the presence of others, etc. In most of the cases it was stated that the child had either been laughed at, shamed or compared unfavorably with some other child. In children a little older, wounded pride in many forms appears. The occasion may be bodily defects, shabby clothing, awkwardness, any reproach of family or station, ridicule or expressions of disgust, social blunders or violations of etiquette. In adolescence, these same causes are frequently greatly intensified and questions of dress, manners, bearing, etc., become matters of extreme importance as sex attraction develops. The moral aspect of shame, which appears to be wholly lacking in the earlier manifestations and more or less confused during childhood, may now become very acute. Yet the fact that the moral quality in shame is not an essential characteristic is evidenced plainly by the fact that our social blunders often cause us keener suffering than do infractions of the moral law. How completely shame is conditioned by the social self is well illustrated in the following case: A boy of 14, whose mother was ill, stayed away from school to help her with the housework. He did it willingly and was rather proud of being able to help so much. But some of his schoolmates stopped to inquire the cause of his absence and found him washing dishes. The situation was immediately changed, and according to his own description, he could not have felt more ashamed if he had been caught "breaking the whole ten commandments" than he did at being caught in the, under the circumstances, morally meritorious act of washing dishes.

In considering animal shames, only those cases were retained as legitimate examples, in which fear of punishment as a possible explanation of the behavior was excluded. One or two examples will suffice to show the character of the material. A pet cat of rather a jealous disposition was shown a life-sized colored paste-board cat. It was rather a realistic representation and being carefully held so that the cat could not see the back of it, was slowly moved towards him. He growled, lashed his tail, and finally sprang at the paste-board imitation, knocking it over and revealing its falsity. Tail and ears drooped and he slunk away under a sofa. He could never again be induced to take the slightest notice of the counterfeit, turning away his head if it were thrust upon him.

That dogs are sensitive to reproach and to ridicule is attested by every careful observer of animals. Shaler gives the following account of a shepherd dog belonging to him

but from which he was often separated by several months' absence. "When after one of these absences I appear to him in the distance he comes furiously barking toward me, quite possessed by his enmity. At a certain point in his charge, a doubt appears to beset him: he moderates his pace; his roaring bark passes into a whine; and as the full measure of his blunder is borne in upon him by my voice, he becomes the picture of shame. In his perplexity, he always finds relief by endeavoring with his paw to scrape a supposititious fly from his nose. He then deals with what I suppose to be an equally imaginary flea. After he has thus gained a few seconds for readjustment, he welcomes me joyously." That reactions of a similar nature occur in a social environment of the animal's own kind is shown by the careful observations of Mr. Ordahl, who thus describes a common occurrence among his cattle when first brought in the fields. "Two young calves are testing their strength, one is pushed out of the ring; the victor raises his head stiffly and jerks his ears alternately; his eyes roll with a gleam of satisfied superiority, while the loser lowers his head, eyes are cast down and the ears hang down relaxed. This lasts for a few moments, when the matter is settled either by the loser cautiously approaching the winner in an attitude of recognized subjection or the winner administering an extra bunt as if to assure himself that the victim is a non-resisting medium."

It is in the social situations arising among gregarious animals that Lloyd Morgan finds the germs of the development of the sense of personality—and Dr. C. W. Hodge, in a paper on "The Intimations of Self-Consciousness in Animals," concludes that we must allow to dogs at least an obscure form of self-feeling. But if we admit even a rudimentary self-consciousness in animals, we have the possibility of an abasement of self which, however crude, may be considered a germinal form of shame and possibly not very different from the earlier manifestations of shame in children. Some investigations of shame in mentally defective children, not yet completed, tend also to show that their capability of feeling shame is in close correspondence with their capability of entering in social relations.

Turning to anthropology for its evidence in regard to the development of shame, we find it, for the most part, confused with modesty. Many of the studies of modesty are bound up with those on the origin of clothing and the chief point at issue is whether modesty was a factor in developing clothing or whether clothing was a chief agent in the development

of modesty. The weight of evidence seems to favor the assumption that psychological modesty preceded physical, and that clothing has been instrumental simply in the development of conventional modesty. This opinion is supported by Ploss, Stevins, Westermarck, Wallace, Foster, Rowley, and Zimmermann, who think that modesty exists quite as truly among unclothed as among civilized peoples. But even in tribes where so far as external evidence goes, modesty is apparently unknown, we find abundant evidence of shame, arising from precisely the same classes of causes or occasions as among civilized peoples. Westermarck, in his "History of Human Marriage," says: "Ideas of modesty are altogether relative and conventional. Peoples who are accustomed to tattoo themselves are ashamed to appear untattooed, peoples whose women are in the habit of covering their faces consider such a covering indispensable for every respectable woman; peoples who for one reason or another have come to conceal the navel, the knee, the bosom, blush to reveal what is hidden. It is not the feeling of shame which has provoked the covering but the covering which has provoked the feeling of shame." Ploss states that it is extremely difficult to obtain exact information in regard to the crippled feet of Chinese women as they are unwilling to allow even a physician to see them. A Chinese woman considers it improper to allow her husband to see her uncovered feet. It is only the tip of the foot encased in its miniature shoe that constitutes the admired "golden lilies" of the Chinese, and the ugly mass of deformity above, which is the penalty of these admired ornaments, is something not willingly exposed. It is possible that we have here a connection between modesty and the æsthetic feeling, since we frequently find the tendency to conceal associated with whatever may be regarded as ugly, repulsive or disgusting.

Darwin in his chapter on blushing ("Expression of Emotion in Man and Animals") records a number of examples of shame among primitive races and notes the sensitiveness of the Maoris to ridicule and cites the case of an old man who, when laughed at, blushed up to the roots of his hair. A Fuegian blushed when quizzed about polishing his shoes and otherwise adorning himself. An Arab on the Nile blushed quite up to the back of his neck when laughed at by his companions for managing his boat clumsily. Rusden in his history of Australia, in describing the corroborees of the Australians says that "the perfection of acting was aimed at by each man in the tribe," and that the unlucky wight who

committed a mistake in the public performance or missed a turn in the air was followed by the taunts of the tribe for weeks afterward. From this it would appear that rules of etiquette are quite as binding and violation of them quite as humiliating among Australian aborigines, after their own fashion, as among civilized peoples. Many other savage customs point toward the origin of shame in the social consciousness rather than in any biological facts.

The development of self-consciousness or the conscious self is for the individual a very gradual process. It begins with the bodily self. For the new-born child the universe is a series of disconnected sensations to which neither the idea of subject or object is attached. There is a pleasure-pain quality in these sensations, so far as may be judged by muscular expression, but the child does not yet know that the body which experiences these feelings of comfort and discomfort is his own. A large part of the first year of life is spent in making this discovery and even then it is by no means complete, as shown by the studies of Preyer, Hall, Shinn and others.

But before the bodily self and not self have been completely differentiated a dawning self-consciousness has already expressed itself in emotion and desire. The more primitive emotions—fear, anger, jealousy, and animosity, have appeared, and the beginnings of a social consciousness are shown in the child's responsiveness to the influences of environment, to approval and disapproval. According to Dudley Kidd this is earlier among the children of civilized than those of primitive peoples. A Kafir child will beat or bite the blanket of a person with whom he is angry and a big boy will cry if his clothing or possessions are beaten. He also confuses himself with his shadow, his name or his picture. His personality too is mixed with that of his clan. This would account for the shame and anger exhibited when the honor of the clan is violated.

With all these stages of the development of self-consciousness we find the various types of shame in man and animals closely corresponding. The immediate causes or occasions of shame vary with environment, and the intensity of the emotion varies as the development of sense of self in the individual. A moral quality appears to be attached only to types sufficiently developed to admit of a conflict between an ideal self and some content of consciousness which is in disharmony with it. But that the feeling of shame is not confined exclusively to the violation of convention among primi-

tive peoples but has an ethical value in some cases highly developed, is shown in the following case of a Navahoe Indian who, wishing to convince his interlocutor of his truth, uttered the following words: "Why should I lie to you? I am ashamed before the earth: I am ashamed before the heavens: I am ashamed before the dawn: I am ashamed before the evening twilight: I am ashamed before the blue sky: I am ashamed before the darkness: I am ashamed before the sun: I am ashamed before that standing within me which speaks with me. Some of these things are always looking at me. I am never out of sight; therefore I must tell the truth. That is why I always tell the truth. I hold my word tight to my breast."[1]

As to the components which enter into the emotion, its physical manifestations and many anthropological facts point to fear, disgust, and perhaps violations of the æsthetic feeling as well as modesty as the roots from which it has developed. Genetically, then, both the physical manifestations of shame and many anthropological observations tend to show that it is a development from the more primitive emotions of fear and disgust, and violations of æsthetic feelings as well as modesty developed as to its special form through the reciprocal action of environment upon the consciousness of self.

[1] Washington Matthews: The Study of Ethics Among the Lower Races. J. of Am. Folk-Lore, Vol. 12, pp. 1-9.

THE ORIGIN OF LAUGHTER[1]

By Sylvia H. Bliss

"The greatest of thinkers, from Aristotle downwards, have tackled this little problem, which has a knack of baffling every effort, of slipping away and escaping only to bob up again, a pert challenge flung at philosophic speculation." Laughter, An Essay on the Meaning of the Comic. Henri Bergson.

There is noticeable in various quarters a revival of interest in the subject of laughter. Thinkers are resolutely attacking this problem—which Bergson may term little, but which in reality is very large. Our knowledge of the exciting causes of this most human act has been greatly extended, yet the subject is by no means exhausted. Despite the clever and penetrating theories advanced the essential secret of laughter remains shrouded in mystery.

Is it not possible that an attack from a different quarter may prove less barren of results? The question may not be for the philosopher after all but instead demand evidence from the biologist and the psychologist. If, through all the centuries, the challenge has been flung at the feet of the wrong party, Bergson may well characterize it as "pert." The problem is not primarily in regard to the nature of the thing laughed at; neither has it to do in the first instance with the reason for laughter at that particular thing. In the act itself is the mystery. Why should we laugh at all?

Those who have investigated the matter have too often paused in their search when a perception of one aspect of the comic has been attained. Thenceforth their endeavors are directed to the task of elucidating and illustrating this one aspect with the result that the original, underlying cause of laughter remains almost untouched. The earnest student turns from current theories in disappointment. They do not go deep enough. Dr. Sidis' recent book, with its wealth of illustration—which his theory, "Allusion to human stupidity is the root of all comic," is strained to explain, leaves the

[1] The writer has learned that the argument in this article is similar in some respects to that used by Freud in his treatise on wit. Her investigation was independent of that of the latter.

reader far from satisfied, while Bergson's clever exposition of the comic is based upon the laughter of highly developed, modern man. From the essay of the latter floats the laughter of the cultured Frenchman—late product of the world's most highly civilized race. For an adequate investigation the consideration of quite other and simpler phenomena is necessary.

The apparent causes of laughter are so various that the generalization which embraces all its manifestations must be wide indeed and likewise very simple. We are forced to reckon not only with the broad and generally recognized field of the humorous and with the stock instances of the comic; with deformity, eccentricity, surprise, queerness, "the mechanical in the living," and the inferior, but also with numerous occasions not readily classifiable which yet can by no means be ignored. The laughter of childhood; the laughter with which, according to a press dispatch, two women in the Italian quarter in New York watched the death struggles of a man whom their companion had murdered; the laughter, as noted by the dramatic critic of *The Westminster Gazette*, invariably won from an audience by the utterance on the stage of the word "damn"—these are a few of the instances awaiting accurate classification.

The evidence afforded by the smile must also be admitted. This fleeting "pale vestige of a laugh," arising at a multitude of provocations, supersedes to a great extent in the highly cultivated person, the more vigorous physical reaction.

Science has demolished the idea of a "funny bone," finds in the brain no special center for appreciation of the humorous, and in the world of thought and action nothing inherently, intrinsically comic. A social corrective, an outlet for surplus energy, relief from strain—laughter is all these assuredly, yet to perceive certain ends which a function serves is by no means to account for its existence. It is evident that the laughter of the modern human being is a highly complex function far removed from the bald simplicity of that first laugh whose strange sound broke the long, silent gravity of the pre-human ages. What emotion gave rise to the first laugh? Was that primeval man in any modern sense amused? He laughed at no sally of wit; his brain was tickled by no subtle and devious perception of the comic. Of this we may be certain. Summoning to our aid the evidence which biology and psychology afford and guided by the imagination exalted by scientific training to a species of insight, we will penetrate to that far away epoch, when mirth was born to earth.

What was the first emotion, and what its cause? Without

doubt the organisms lowest in the scale of life were and are emotionless. Fulfilling without hindrance or delay their functions of nutrition and reproduction, with need and opportunity for satisfaction, income and expenditure, nicely balanced, what room for pleasurable or painful feeling? Scarcely with accuracy may we use the term satisfaction in connection with forms so simple, for satisfaction implies a period of want, and here such hiatus between desire and fulfillment, if existent, is infinitesimal. With advance in the scale of life this simple equilibrium is disturbed. Instinct is delayed in its functioning, hunger and reproduction must wait on occasion. Thus began the swing of the pendulum between need and satisfaction, and without doubt all the marvelous complexity of human emotion is due in the last analysis to the lengthened repetition, the slower and more devious recurrence, of that primal oscillation. With further advance need grew more poignant, satisfaction more intense, until in the higher animals we meet states of feeling fitly termed sad and joyful.

Why not laughter? Why is man alone, of all creation, the animal that laughs? Readily enough comes the answer: Because man alone has intelligence to perceive the humorous. But we find that intelligence is not coextensive with laughter nor is appreciation of the comic a necessary condition of its arousal. Yet it is possible that a consideration of certain other differences between men and animals—differences usually overlooked—will yield matter of greater significance to our problem.

The animal is perfectly natural. It follows instinct, hiding and repressing nothing. It may growl, roar, fight, give chase, plunder, excrete, and, subject to certain limitations, feed and reproduce, when it wills. Indecency and shame are words without meaning. Rightness exhorts no homage. If he loves, well and good, but if he hates no social or ethical code torments him with its "ought." In scientific phraseology there is for the animal no inhibition of instinct. With this free and natural state contrast man's condition. In the evolution of humanity those instincts which are inimical to the progress of civilization are theoretically transformed into qualities and acts less at variance with social and ethical laws, but in reality the substitution is far from perfect and entire. Man is not yet completely evolved; he is but partly adjusted to a civilized environment and a portion of his nature lags far behind at a primitive, savage level.

The human being, from childhood up, must curb, repress, skulk, hide, control. From the mother's "no, no," to the

thundering "Thou shalt not" from Mount Sinai there is a constant denial of instinct. So accustomed are we to regard this as pure benefit that we are blind to the accompanying disservice.

Consider man's inner life—a nest of hopes, impulses, and desires, in themselves perfectly natural and to be expected at this stage of evolution, yet directly in opposition to the prevailing social, ethical and religious codes. Nature confined is not entirely quiescent. With all the outlets, transformations and substitutions which physical and mental activities afford there remains still a large residue of repressed primal instinct which results in discordant and tense conditions in the subconscious life. The repressions of the primitive man of our study were obviously of the most elemental impulses and took place in obedience to nascent social demands of the tribe and through his own growing sense of shame.

Regarding emotion in its essence as the state of tension occasioned by delay between impulse and act, desire and fulfillment, hunger and satisfaction, and perceiving that the primary emotional difference between animals and men consists in the fact that the latter, of their own volition, to serve certain ends, carry on the work of restraint and delay initiated by nature, eventually pushing into the subconscious region tendencies at variance with the slowly forming social code, we discern the conditions ripe for the origin of laughter. For laughter is the result of suddenly released repression, the physical sign of subconscious satisfaction. Our primitive man saw, it may be, another do the thing reprobated by the tribe and his own nascent conscience. The sight relieved the tension occasioned by his own repressed wish to do the selfsame thing—and he laughed. That paroxysm of nerve and muscle was not caused by sense of superiority, as Dr. Sidis would no doubt claim; rather it sprang from unconscious sympathy with the reactionary act.

It is probable that the track for that particular discharge of nervous energy was laid long before actual laughter took place. Darwin noted that the lower jaw of some species of baboons quivered up and down when they were much pleased. The writer has seen the jaws of cats quiver when the animals were excited by the near approach of prey. Expectancy, pleasurable anticipation, satisfaction, with their accompanying bodily reactions, paved the way for laughter.

Today in the highly civilized human being what part of the personality laughs? The highest and latest evolved? The keen trained intellect perceiving something which delights it?

Do wisdom, learning, dignity, purity, laugh? Recall Meredith's opinion as voiced by Adrian Hartley: "Mournful you call it? Well, all wisdom is mournful. 'Tis therefore that the wise do love the Comic Muse. Their own high food would kill them. You should find great poets, rare philosophers, night after night on the broad grin before a row of yellow lights and mouthing masks. Why? Because all's dark at home." From what region does that broad grin come? From idealism, philosophy and wisdom satisfied by nonsense, pleased at folly? Or does the laugh spring from that in the man which is akin to what is portrayed?

We are but half in sympathy with that which we profess. We are not in all respects what we think we are. Under certain conditions there is release of our unrecognized tendencies. De Quincey said that a man is disguised by sobriety rather than by intoxication; and as intoxication uncovers the under-self in man, so laughter, in the temperate, discloses, more delicately and deviously, the mind's unconscious tendencies. Grave, dignified, humane, respectable, religious —why do we laugh at the clever portrayal of characters quite the opposite? Is it not because a part of our personality is in sympathy with the indecorum, triviality, vulgarity of what we see? Are we not as honest at the vaudeville as at church —if we attend both? Even more honest? There may be hypocrites at church; never at the theater. We may bow the head during public prayer because it is the custom; we laugh at a joke because we must.

This view finds confirmation when we consider what occurs during a struggle to suppress laughter—at the indecorum of a child or at a joke "delicately not decent." Our surface sense of propriety strives to suppress the evidence of subconscious approval. Without doubt the blush is the result of similar though still deeper, conflicts.

A clever comedienne responding in a recent interview to the query, How is one funny? said, "I believe that the thing that really takes is the thing with a touch of nature in it." Her remark was more profound than she knew. The secret of laughter is in a return to nature. Civilization and culture are late additions and we are living to a great extent in artificial conditions. Even common sense, according to Bergson, is an effort. Psychology makes plain the fact that our present mental equipment has been slowly and painfully acquired and a certain strain in maintaining that high altitude is inevitable. This tension is relieved by nonsense and by the portrayal in humorous anecdotes and on the stage of evasions of con-

vention and infractions of the prevailing code of manners and of morals. Carlyle once declared that Shakespeare was greater than Jesus and pointed to the creation of Falstaff as proof of his assertion. Of Christ he said: "There is no Falstaff in Him." For the individual who is not shocked by this remark it is a perennial source of humorous feeling. I defy a broad and healthy-minded man, even one of fine religious temper, to read it without smiling. Casting about for a reason what do we find? Contrast, the inferiority of Falstaff, the fact that he is a stock character, typical of certain vices? Various formulas of the comic fit the case, but do not, I venture to say, explain the peculiarly humorous character of Carlyle's statement. Falstaff stands as a representative of the primitive, natural man, and brought into juxtaposition with his most startling and exalted antithesis in character, elevated for once in human judgment above that which has stifled and repressed the traits he represents, he triumphs for the moment, and through him nature in ourselves justifies its existence—and laughs.

I am not aware that any explanation has been given of the smile of the subject entering the hypnotic state. As this is a usual accompaniment of the condition it presents an interesting problem. If the theory advanced in this essay approaches the truth the solution of the problem is made plain by Professor Jastrow's description of the hypnotic consciousness: "— a release from the restraining influences of fear, hesitation, and the ideals of reason and propriety." Here again a simpler, more primitive self has its opportunity—and smiles.

Having stated, somewhat clearly and convincingly, it is hoped, the theory of laughter as the expression of subconscious satisfaction, it remains to test the theory by application to various occasions of laughter. Before proceeding, however, a glance at Bergson's essay on the subject will be instructive. As I advanced in my study from point to point, the main thesis becoming at each step ever clearer and more reasonable, I was struck by the confirmation afforded it by certain isolated passages in the work of the French philosopher—passages which he interprets in accordance with his definition of the comic as the "mechanical in the living." "What is essentially laughable is what is done automatically. In a vice, even in a virtue, the comic is that element by which the person unwittingly betrays himself—the involuntary gesture or the unconscious remark." "Profoundly comic sayings are those artless ones in which some vice reveals itself

in all its nakedness." "Any incident is comic that calls our attention to the physical in a person when it is the moral side that is concerned." "The comic character slackens in the attention that is due to life." "The comic is that side of a person which reveals his likeness to a thing."

Why should we laugh at automatism, the mechanical, absent-mindedness? Bergson's explanation is that these things stand for unsociability, for imperfection, which call for the corrective of laughter. But we do not laugh in order to correct the comic. Laughter is spontaneous and involuntary. It bursts forth when a person unwittingly betrays himself because that betrayal is of natural, elemental tendencies. Laughter is honest and responds to the primitive honesty of others. We laugh at a sneeze which hinders a speech at its most pathetic passage because pathos in oratory is always somewhat strained—even artificial, while the sneeze is absolutely natural and honest. We laugh at the sudden intrusion of the physical when the moral, spiritual, or intellectual is predominant because the latter are of late date in man's evolution—from the body's standpoint interlopers in the household of personality, and the abrupt resumption of rule by the physical delights because there is relief, triumph, even, fancifully speaking, a species of revenge. What has not the body suffered through dominance of the soul!

Passing to the consideration of some of the conditions which give rise to laughter it must be admitted at the outset that not all upon analysis yield to the interpretation advanced in this essay. But let it be remembered that our theory endeavors to account for pure, elemental laughter. Once organized, once this particular emotional reaction—possibly accidental in the first instance—became a habit of the nervous system, occasions for its use multiplied. The act was of value to developing humanity. It has eased—it is easing—the transition from the freedom and naturalness of animal and savage life to freedom *from* the animal and savage, our long and difficult task.

All theories of laughter and the comic are more or less applicable for the laughter of the world today flashes and gleams like the rays of light from a many-faceted jewel. Still do we find it in large measure the symbol of subconscious satisfaction but it has become expressive also of various other satisfactions, complex, subtle and devious. Certain of these are direct outgrowths of the more primitive forms and traceable to the same roots, while others are apparently purely mental in character. It is notable that the higher we rise in

the scale of wit and humor the less hearty and boisterous is the accompanying laughter. It has been said that it is with their minds that Americans laugh most, nowadays. Interpreted from the standpoint of our study this indicates an advance in evolution—the substitution of mental for more primitive needs and tendencies.

Obviously our formula covers all instances of the humorous which relate to socially and morally reprobated acts and conditions. It is in this region that the greatest repressions occur and these in turn give rise to our broadest and coarsest humor. The theory likewise accounts for laughter at remarks having double meaning and at jokes whose point depends upon their vague hint or suggestion of evil. These may win a smile from persons who would turn in disgust from obvious vulgarity and obscenity. Only a keen and delicate shaft can penetrate their surface conventionality and refinement. But a step removed is the laughter which greets profanity on the stage. Without doubt the laugh springs from unconscious sympathy with the vigorous expletive. The theory easily fits also that large field of the comic which owes its power to the sudden humiliation and confounding of pride, pretension, formality, dignity, assumption of piety, learning or virtue, and the like. We are subconsciously on the side of the child or simple, unconventional individual who blurts out the plain, honest truth.

Beyond this lies a very wide region whose exploration exceeds the purpose of this essay, yet one or two ventures beyond the easy and obvious application of our theory will serve to show its unsuspected scope and adaptability. Take, for instance, the story cited by Bergson, and quoted by Dr. Sidis in his *Psychology of Laughter*. The anecdote relates to an M. P., who, when questioning the Home Secretary on the morrow of a terrible murder which took place in a railway carriage, remarks: "The assassin, after despatching his victim, must have got out the wrong side of the train, thereby infringing the Company's rules." Bergson explains the laughable effect of this remark by the implied automatic regulation of society, "an administrative regulation—setting itself up for a law of nature." Sidis dismisses it with the brief note—"There is nothing mechanical about it except the fact that the remark shows the stupidity of the M. P." But just where is the real crux of the story? Do we laugh at the implied automatism, or, on the other hand, at the assumed stupidity of the M. P.? Rather is not the smile caused by the sudden subordination of artificial, man-made rules by the

great, tragic, unconventional, and in a sense natural, act of murder? Shift the order in which the incidents are related, picture first the rigid regulations and then their sudden infraction by the escaping murderer, and we have tragedy, the direct predominance of the greater fact. But tune the imagination first to murder and then obtrude the petty rules as in any sense binding upon that wild, tragic,—even triumphant figure, and the wild, tragic and unconventional within ourselves exults in its superiority and priority,—and laughs.

Or, again, why do we laugh at the following anecdote related by Sidis? "An Irishman in a museum was looking at a copy of the Winged Victory, and asked an attendant what it was called, 'That is a statue of Victory, sir,' was the reply. Pat surveyed the headless and armless statue with renewed interest. 'Victhry, is it?' he said. 'Then begorry, Oi'd loike to see the other fellow.'"

Undoubtedly Pat was ignorant—of art, yet it is not this fact in itself which renders the story so delightfully refreshing. Spencer's theory of the cause of laughter as a descending congruity—the transference of consciousness from great things to small—is in order here; also the explanations offered by contrast, surprise, superiority, and Bergson's idea that we are amused at the intrusion of the body when the soul is meant. Yet these theories merely remove the problem a step farther from us. Why do we laugh at descending congruity, contrast, the body when the soul is meant? In attempting to apply our own explanation let it be noted that the story amuses in proportion to our appreciation of the value of the statue—as a work of art and as a symbol of man's highest victories. Is it that portion of ourselves in sympathy with the highest meaning of the work which laughs? Or is it that submerged nook where lurks approval of Pat's point of view,—which delights in his vision of a vanquished physical antagonist, minus even more than head and arms? Doubtless we harbor an unconscious pugnacity revolting in obscurity against the dominance of unfleshly ideals and which, through the medium of the story, is for the moment triumphant.

Passing finally to another variety of the comic, why do we laugh at the story of the Irishman who declared that the moon was of greater use than the sun because it shone at night, when without it we should be in darkness? The secret spring of amusement does not lie in the fact that the speaker is ignorant, for, state the matter in another manner,—say, "There is a man who does not know that the light of day

comes from the sun and that the moon's radiance is but a reflection from the same source," and we smile but slightly. Evidently the mirth-provoking element is to be found in the manner of making the statement. We laugh at the first form because we perceive the fallacy of the statement instantaneously, without a conscious process of reasoning. We delight in mental agility, just as on a lower plane we delight in bodily agility which in turn is a substitute for still more primitive activities.

There remains to be explained the tendency to break forth into laughter on occasions of great gravity, solemnity, sorrow, and even tragedy. Constituted as we are it is evident that the entire personality can not long remain keyed to the highest pitch. There is always the possibility of dissociation, of the sudden breaking away of the natural man under the strain,—or perhaps more truly the severance of the tense, solemn mood, resulting in relief and satisfaction on a lower plane. The mirth of the two women witnesses of a murder, before mentioned, may be referred to subconscious cruelty, and this in turn—it is probable—to still deeper repressions and perversions.

Apparently the natural method would have been to investigate first the laughter of childhood, but if the view advanced in this essay approaches truth it was not the child who first laughed. Repression of impulses and conscious regulation of the life could begin only with the adult. In time the artificial standards would be imposed on the child and at length inhibition would become organized in the race—a habit of the nervous system. Laughter, beginning as an accidental physical convulsion following indirect or delayed satisfaction of repressed impulses, would likewise have become a habit of the body and it is not difficult to conjecture why in the child, that bundle of budding instincts and premature impulses laughter should follow the slightest stimulus. Later, as body and mind are increasingly exercised, furnishing an outlet for subconscious impulses, laughter becomes less.

In a recent interesting article H. Addington Bruce[1] describes the laughter of childhood and in consequence all laughter as "a means whereby nature provides a salutary outlet for surplus nervous energy." But it is doubtful if we can, with strict scientific accuracy, speak of nature as deliberately and directly implanting an instinct "for the performance of an important physiological function." Among the various responses of a developing organism to environment those prov-

[1] *The Outlook*, August 9, 1913.

ing useful would persist as habits and eventually become organized as instincts. The habit of laughter developed and survived because it was useful. The man who laughed heartily was among those fittest to survive. Furthermore, "surplus nervous energy" is but another name for unused power which could arise, in the first instance, only through the restraint and repression of natural functions. Thus it will be seen that the theory of laughter as a means for the outlet of nervous energy itself requires explanation, and this explanation is afforded, logically and satisfactorily it appears to the writer, by the doctrine of the satisfaction of subconscious tendencies.

It may be objected that the view of human nature involved in this theory of the origin of laughter is degrading. From the standpoint of the idealist the assumption of a special faculty of the mind whose function it is to recognize the presence of a definite humorous quality in events and acts would better accord with man's dignity and worth. But the evidence favors no such assumption. On the other hand, those who delve most deeply into the abyss of human mentality testify to the complexity, contradiction, and inequality of the elements of mind. The recognition of truth is in the end never degrading but prophylactic and remedial. Perceiving the function of laughter-provoking agencies we shall be slow to condemn even the broadest and coarsest humor, for this, furnishing an indirect outlet for suppressed instincts, may be more beneficent than we know. Lacking this relief the impulses might seek satisfaction in forms much less innocent. Laughter is born of the exigencies of evolving humanity and it will be long before its joyful echoes die from the earth.

THE FORM OF THE CURVE OF PRACTICE IN THE CASE OF ADDITION

By EDWARD L. THORNDIKE, Teachers College, Columbia University

Each of 670 college students added daily for seven days forty-eight examples like those printed below, using seven printed sheets differing in the examples, but equal in average difficulty.

7	4	9	2	3	7	2	7
8	2	3	9	7	4	6	5
6	3	6	6	8	8	3	2
5	2	6	3	5	3	8	7
6	9	5	7	3	3	3	5
5	5	8	2	2	7	9	8
3	8	4	9	8	5	9	6
5	7	8	6	3	7	5	4
7	5	3	3	9	8	2	8
7	5	2	8	8	5	5	3

Each individual kept score of the time required and of the errors made. The scores for time required were kept with ordinary watches and are probably not very exact and are probably occasionally afflicted with large errors (of a minute or more).

I show in Fig. 1 the practice curves for the 336 columns, for the average of the 7 who were initially slowest, the average of the 13 next slowest, the average of the 29 next slowest, and so on, with groups numbering 36, 42, 61, 64, 45, 42, 73, 45, 41, 56, 52, 22, 22 and 21. These practice curves with two exceptions show the straight slope found by the author in a similar experiment with 19 adult students. (The two exceptions are the curve for the two most rapid groups. In the most rapid group there is a falling off with practice.[1])

[1] The measurements which this curve represents are for special reasons much less reliable than those for the initially slower groups; and its eccentricities should not be taken too seriously.

In the next most rapid there is zero improvement at first and then a very rapid rise. The results are thus notably different from the average curve found by Wells with hospital attendants, and from that found by the author with children in the fourth grade. In these two latter cases the curve rises rapidly at first and then more and more slowly.

I am inclined to explain the difference between the straight slope characteristic of college and graduate students, and the parabolic form characteristic of hospital employees and young

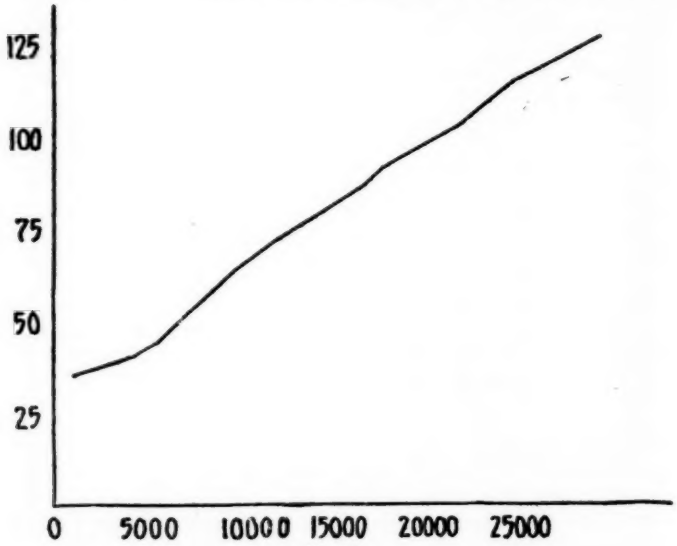

FIG. I.

school children as a result of differences in interest. This, however, is speculative. What is empirically shown is that the difference is *not* due to the fact that the college and graduate-student groups begin their practice at a much higher level. For the lower groups of these 670 students began at a lower ability than Wells' ten subjects; and, further, within the group there is no tendency for the initially lowest to show either a more rapid initial rise or greater negative acceleration than the initially higher group. If we should make a composite practice curve for improvement from 34 additions per 100 seconds to 150 additions per 100 seconds,

by taking the curve of the slowest group until it reached the height of the beginning of the next slowest group, then extending it by taking the curve of this next slowest group

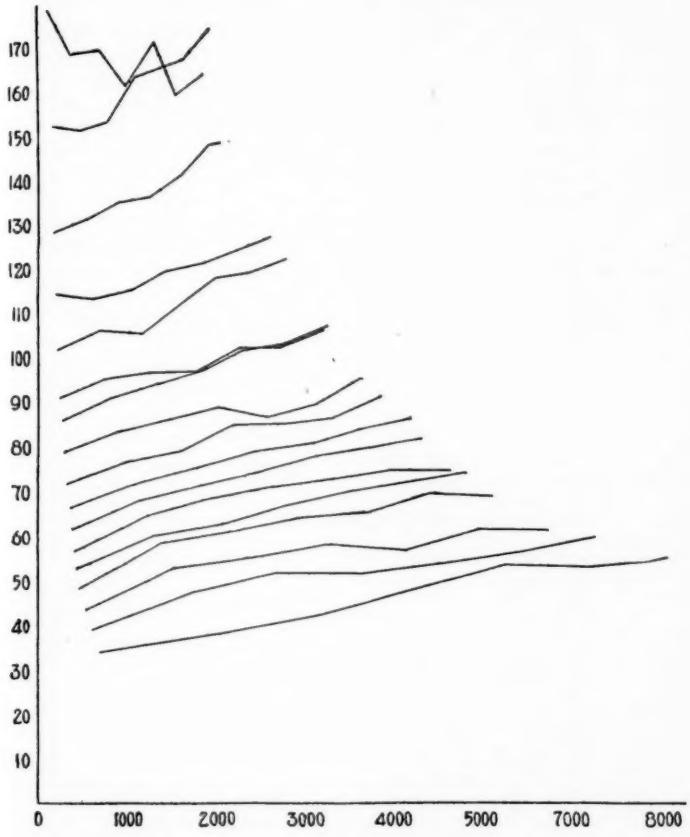

FIG. 2.

from its beginning until it reached the height of the beginning of the third group in order of slowness, and so on, the composite so obtained would not be at all of the parabolic

form. It would be very close to a straight line, as is shown in Fig. 2.

Description of Figures 1 and 2

Fig 1. Practice Curve in Single-Column Addition. Each curve represents the average of a number of individuals, the numbers being 7, 13, 29, 36, 42, 61, 64, 45, 42, 73, 45, 41, 56, 52, 22, 22 and 21 in order, beginning with the lowest curve. Distances along the base-line represent amounts of time devoted to practice. The vertical heights of the curve represent the number of additions per minute (writing the sum being counted as one addition).

Fig 2. Composite of All save the Two Highest Curves of Fig. 1. The lowest curve is taken from its beginning until it reaches the height of the beginning of the next lowest curve; then this next lowest curve is taken until it reaches the height of the second from the lowest curve; and so on.

GHOSTS AND THE PROJECTION OF VISUAL IMAGES[1]

By LILLIEN J. MARTIN, Leland Stanford Junior University

Some tests recently made with three Stanford students who reported that they had seen apparitions show most clearly that whether we will see ghosts at all and what their appearance will be depends upon whether we normally project our visual images into space or are able to do so under the influence of a strong emotional stimulus. I give first, in the following, what the students gave to protocol in regard to the apparitions they had seen, and then briefly summarize the results of the tests I have made with them regarding their ability to project visual and other images.

I. Mr. M., special student in Chemistry.

"Time of appearance of the apparition—the first Sunday afternoon in May, 1908. Quite normal in health. I was reading with much interest in Mendeléeff's 'Principles of Chemistry.' That I was awake I am quite sure, as my sister had just passed through the room and we had spoken. Quite suddenly the book seemed to disappear, and in its place came the appearance of a man, head and shoulders. He wore a battered sombrero and soft dark shirt, open to the second button. About him was the seeming of brilliant sunlight and air of crystalline purity, and the sense of high places and much living out-of-doors. Though I could not see it, I knew that he carried a gun, probably a rifle, under his right arm, possibly suggested by the set of the shoulder. The face was attractive at first glance, rather handsome—or easily might have been. The skin was clear and beautifully bronzed. He looked back at me with a clear, open, frank gaze, with a half-smile on his face. But about him there was a swagger, a braggadocio, an insistence upon his own importance and his will to dominate without much consideration for the other fellow, that, though it was not consciously emphasized on his part, irritated me and left me somewhat ill at ease. He remained quite two minutes, while we stared at each other. The smile, somewhat superciliously amused, grew broader, and then slowly he faded away and the book blurred back into place. I finished the sentence and the paragraph.

Time, June 2nd, 1908, about midnight. I had been out to call, had particularly enjoyed the evening and had stayed late. I was sitting

[1] Martin:—Die Projektionsmethode und die Lokalization visueller und anderer Vorstellungsbilder. Zeit. f. Psych., Bd. 61 (1912), p. 321, and published in book form by Barth, Leipzig.

on the edge of my bed unlacing my shoes. Suddenly I sensed another personality present and looked up quickly in surprise and inquiry. The same man stood across the room from me, perhaps ten feet away. The entire figure was visible. He was dressed much as before, but without the hat or the gun. If before his self-assurance had irritated me, there was now about him a conscious, purposeful, insistent masterfulness that I would not and could not endure. It aroused an instant and fiery antagonism. My feeling was—though I did not say it—"You get out of here, quick!" Yet I had a decided impression that he wanted something that I might give. But he did not ask, but demanded, in impossible ways. It seemed, too, that his manner was a pose, assumed as much to impose upon himself as upon me. A bit of unacknowledged fear lurked somewhere back in his consciousness. All this, somehow, I knew. He stared at me a moment, with his insolently irritating smile, took three steps to the side and diagonally toward me, and suddenly disappeared. The incident left me so irritated that I slept little. Early in the morning of June 3rd I left for the "High Sierra" on a two months' camping trip.

About three weeks later, when we reached the upper canyon of the Kern River, I had a very strong and quite unreasonable, persistent desire to go farther up the canyon, which was not possible at the time. This desire, after a day or so, associated itself with a strong sense of the personality of the man described above though I saw nothing. So compelling became the desire under the stimulus of this association that one night, somewhat after midnight, I walked almost without volition of my own, perhaps six miles up the trail in the moonlight until an impassible stream halted me. I seemed to *know* that this man was there up the canyon, and because he was there was I compelled to go.

We camped, three days after this experience, at the edge of Monachi Meadows. About midnight I was awakened, very suddenly, by someone shaking me roughly by the shoulder. I sat up. The same man stood beside me, hatless, but with his gun. I saw the glint of the moonlight on the gun barrel when he moved. He seemed to be in overwhelming terror. The qualities in him that had so irritated me before were quite gone,—the braggart was become the craven. I knew, somehow, that he came as a supplicant. He said nothing, nor did I. After staring at me a moment, his head drooped in sudden hopelessness, and he walked away.

On the first occasion I could not see the book through the man just described. The second time, he was transparent, so that a chiffonier back of him was quite visible. So, too, the third time, I saw the trunk of a tree through him. That is, he was opaque the first time, but semi-transparent on the two other occasions.

In 1901, in connection with philosophy course, given by Dr. Lovejoy, I read much Hindu philosophy; and I remember that Dr. Lovejoy talked in his lectures of Egyptian religious beliefs, among others. I was much troubled that spring by a series of apparitions. I do not remember just when or under what circumstances these apparitions commenced, nor how many times they occurred, but they occurred very frequently. The details of each occasion of their appearance, what they did and said, I do not recall. One frequent astral visitor was a Hindu, seemingly a pundit or yogin; another, less frequent, was an ancient Egyptian, whom I much disliked. Upon the appearing of these apparitions I would be first conscious of the eyes, from which point the rest of the figures would slowly develop downward. Their

disappearing was the reverse process, from the feet to the eyes, which would persist sometimes much longer, at times for hours. The figures always were semi-transparent, though very distinct, so that objects were quite visible through them. They frequently talked to me. The Hindu, particularly, would deliver long discourses to me which at the time seemed the profoundest of wisdom, but which, upon later analysis, I always found to be a vague jumble of meaningless phrases. At first I permitted these apparitions because they amused me, and I could banish them at will. But later they became more persistent and I could not rid myself of them. Particularly the eyes would appear and follow me, without the rest of the figure becoming visible, which caused me much annoyance and distress. I remember quite distinctly the last occasion on which I saw them. I was alone in the country. Upon arising in the morning about 7:30 I went to the piano to play. Suddenly I saw the eyes of the Hindu above the piano. Of what happened thereafter I know nothing. I found myself at 9:30 in the evening exceedingly weary, still sitting upright at the piano. I do not know whether this was normal sleep or of a hypnotic nature. I had had some slight experience with auto-hypnosis, having on several occasions put myself to sleep by looking at a bright object, first resolving to awake in five or ten minutes.[2] I was so frightened by this experience at the piano that I thereupon determined never to see these particular apparitions, or their eyes, again, nor to go to sleep without my own volition, nor have I. I have, however, occasionally seen other eyes. For instance, a few evenings ago I was reading in bed when a single eye, about four inches long, appeared back and at the right side of my book. I glanced at it and continued my reading. Presently the eye came nearer and slid over the edge of the book. It was transparent, and the words were quite visible through it. It persisted about ten minutes. In general in looking at persons I am quite conscious of their eyes, as the eyes are to me the most noticeable and important feature of the face; just as the head is of greater importance than the rest of the figure to me. In looking back at this whole experience I see clearly that it is all to be explained by the projection of visual and other images, perhaps with auto-hypnosis as a subordinate factor.

As a child, from my earliest recollections to the age of about twelve, I had a considerable number of imaginary playmates who seemed very real to me and of whom I was very fond, so that I cared little to associate with other children. To each of them I gave a name, and each was a distinct and well characterized personality. When they finally ceased one by one to come to see me I often regretted their absence and wondered what had become of them. It was therefore, a matter of great surprise, in 1905, to meet on the street in Palo Alto, a young man (Dr. A. L. Münger, Jr.), whom at once I recognized as Futoni, one of those friends of my childhood. The fancy still persists that the two are identical. And I have since met two other men who just as surely are identical with my imaginary friends. These expe-

[2] We see from this experience of Mr. M. that with him as a reagent we could doubtless easily have developed, artificially, at that time, a state of "Trance," and thus have been able to study the origin and evolution of a "medium." It would not have been necessary at the time of this occurrence to build up the "controls" by means of suggestion for they were already at hand in the persons of the Egyptian and Hindu.

riences of my childhood made me immediately accept as self-evident the theosophical idea of reincarnation when it was by chance presented to me in later years. That this particular group of apparitions were projected visual and other images, I am satisfied. As to their character, I am still in doubt, for my early environment does not seem to me to adequately account for their content."

I add still one more of Mr. M.'s projected visual images. "After spending the afternoon of Wednesday, March 18, 1914, in the petrography laboratory working at the same microscope with Mr. H., I called upon Mr. W. in Palo Alto. As Mr. W. and I were walking to the street-car about six o'clock, I saw a transparent image of Mr. H. walking just in front of Mr. W. on my left. When I left Mr. W. the image of Mr. H. followed me aboard the car, sat by my side, and went home with me. I confess I had a strong impulse formally to present him to my wife. The image sat in a chair in the living room when we went in to dinner. He was in the same chair after dinner, remaining with the family for half an hour. When I went upstairs to read the image followed me and remained until nine o'clock, when he walked out of the room."

Tests with Mr. M. in the laboratory, show: (1) That his visual images are strong and that when he casually thinks of an object such images are located in the back of his head, but if he thinks of the object in detail the image of it is out in front of him, the distance away depending upon the circumstances under which the image is seen. (2) When he projects visual images of persons he has seen, they are translucent. He does not usually see through such images but can easily do so. The transparency of his hunter, etc., images, was doubtless due to the presence of the bright light which was so emphasized as to divide the attention as, for example, in the case of seeing of the "glint" of the moonlight on the sun. (3) When he thinks in the laboratory in detail of the hunter, the projected visual image which appears has the same general appearance as the corresponding apparition except that it is slightly less transparent. The projected image in the laboratory seems "hardly as real" as his apparition, however. He feels it is an image of his apparition, that is, that it seems to have the same relation to it as does the projected image of an object. The emotional experience with its kinaesthetic accompaniments localized largely in the back of his neck and arms (he has a very slight lesion in the back of his neck which produces at times, a feeling of numbness and a dull ache in his arms), which are elements of the feeling of reality, are there but they are much decreased in strength. This hallucinatory experience as well as others like it show that it is not alone necessary to be able to project one's visual images and to do it usually or very frequently in order to have a hallucination. Mr. M. does on occasions when he has no hallucinations.

Other persons who have strong visual images and usually project them have never seen an apparition. The hallucination image must have a content or be accompanied by an emotion which separates it out from other projected visual images. (4) Mr. M.'s auditory images are projected. When he thinks of a sound he hears it again and it seems to come from where it originally came. (5) He can project touch images very imperfectly.

II. Miss T., a student in the German Department, graduated at Stanford University in December, 1913. Early in December I was told that Miss T. had seen a ghost. I give below a curtailed account of the interviews I had with her concerning the apparition she had seen.

December 8, 1913, evening. My own study. Miss T. said:—"I was awakened with a start on the night of December 5th, at 2:15 a. m. When I opened my eyes I saw my ghost, which I had often seen before, standing in my window. He has a high forehead, very hollow eyes, which appear to be only black blotches. His eyebrows are very dark; nose large; very sunken cheeks, and a very prominent chin. He wears a long white flowing robe. His hands are thin and bony. I lay in bed and looked at him. He did not move. He had a long chain in his hand. My bed is next to the window, so by putting out my hand I could have touched him. Finally I sat up. When I did so, he vanished out on the porch (which is off of my room) and sat on a couch out there. I got up and turned on my light and I could still see him, just as plain as when it was dark. Then I took my eyes off him and looked in the mirror to see if I were awake. Then I looked back and my ghost was still sitting in the same position. I crossed my room and turned on another light. He followed me, keeping about three feet behind me. I looked for the feet of the ghost but could not see them, on account of the long robes. Then I went out in the hall into another room. He followed. While I was in this room he stood in the open door. When I came out he stepped out of the way and allowed me to pass. He followed me until I reached my door and then vanished."

"Were you afraid of him?" "No. He fascinates me. I never want him to leave."

"Why not?" "I have always felt he would bring me good luck."

"Why did he come at this time?" "I do not know. But about five days before, our housemother had remarked that it was about time for my ghost to appear."

"Did he speak?" "No."

"How do you explain this ghost?" "I have always explained this as an hallucination, yet I have a feeling that it is a spirit and have always felt this. I feel that it comes from another world, of which we know nothing."

"What do you mean by an hallucination?" "It is a visual image so firmly fixed in one's mind, that it appears without any conscious effort on the part of the person."

"Did the ghost bring any message?" "No."

"Has it ever brought a message?" "No."

Room was darkened. "Can you see the ghost now?" "No. But I saw a large rectangle of white."

Second trial. "Even feeling that it would be a good omen, didn't make it come." (She has just written me that she has after many trials succeeded in getting a visual image of the ghost but only with closed eyes.)

"Give an account of previous experiences with this apparition." "It first appeared when I was about twelve years old. It was my idea of a ghost, from stories. It has appeared since on the average of twice a year. The apparition always appears in the window and grows more distinct each time. The other night it was closer to me than it ever had been, except in one instance. On this occasion, it came and sat on my bed, and was about to speak to me, when I called my roommate. The room was dark. The minute I spoke it vanished. I am sorry I called because I am sure it would have spoken to me. This ghost is always the same in appearance but always carries something different. I remember that once it had a large red book, another time an umbrella, and another time a key."

"Have you ever had any other similar experiences?" "No."

"Did you read ghost stories as a child?" "Only a few. My mother did not approve of my reading them."

"Was there anyone who encouraged you to read them?" "My aunt occasionally told me ghost stories."

"Is there any one in your family who believes in ghosts?" "No."

"What is your physical and mental condition in general when the ghost appears?" "It comes at times when I am physically well, happy, and not mentally tired."

Tests:—Instructed her to recall in turn, Dr. Jordan, Miss McCracken, etc., and to see them out in the room before her. She reported that she saw each person distinctly.

"Do they appear solid?" "Yes."

"Can you see Mrs. Herd?" "No, I don't know her very well."

"Can you put a chair at the side of this real chair?" "Yes: It is almost as real as the chair itself."

Pointing at a reproduction of Raphael's Sibyls, hanging on the wall, "Can you place at the side of that picture another like it?" "In general composition, yes, but not in detail." Repeated with Böcklin's Selbstportrait. "I can get Böcklin and the frame and mat clearly. The frame, however, is not as clear as the chair was."

Miss T. remarked during this visit that she was going to question the ghost next time. I suspect it will reply for trial showed she has the ability to project auditory images. Her images of touch are also projectable. She says this ghost is a proof or confirmation of what all her reading has more and more convinced her, namely, that there must be another world. Recently Miss T. has sent me some notes found in her diary in regard to the ghost. February 2, 1901:—"My dear old ghost was here last night. He had a Latin Grammar with him. I hope he doesn't have to struggle as I do with the subjunctive." July 15, 1901:—"The idea of carrying an umbrella in the middle of summer! That's all the sense my ghost has. He came bouncing in with an umbrella last night. He closed it as he entered through my window. He was so busy seeing if he could find any holes in my stockings, that he forgot to take his umbrella with him when he left. I couldn't find it any place in my room this morning, though I looked everywhere. I guess he came back and got it when I was asleep." January 9, 1902:—"My old ghost amuses me so. Last night, he did a fancy dance at my window. Then he came and rested in my rocking chair. The nerve of him! As soon as I spoke he vanished."

In my office two days after the tests mentioned above when I repeated some of them with like results. Miss T. gave me her reasons for not believing the ghost is merely a projected visual image:—1. The fact that the ghost came when she was well made her feel it was a real ghost for she supposed projected images came when one was tired or not well. 2. The fact that she was not afraid of it also made her feel it was real. 3. She cannot project her ghost at will or make it come at night when she wished. 4. She said, after I had tested her regarding her ability to project her visual images, that she had thought in the past that the ghost was more real than a projected visual image but recent trial with projecting the image of a very intimate friend had convinced her that the ghost was no more real than the image of the friend. She added that neither the ghost nor her friend's image follow her eyes in turning her head. The ghost, she also said, followed her when she walked away but her friend did not and then added, "she is very deliberate."

It will be seen from what has been just said, that Miss T. is ordinarily able not alone to project her visual but also her auditory and tactile images and yet she is not able to project her ghost at will. This fact shows that the ability to project one's images is but one of the factors involved in the seeing of ghosts. In the case of this ghost there is not only a favoring emotional factor which is connected with its appearing but one of an inhibitory character which interferes with its arising except on propitious occasions. The favoring and inhibiting emotional complex which has prevented a very lively visual image of childhood from disappearing is doubtless of a religious nature. The ghost coming, as its seer believes, from another world, is a source of intellectual comfort. Through its presence she has been enabled to successfully resist that skepticism regarding the existence of another world which she thinks might otherwise have been engendered by her University studies.

III. Mr. ———, student in the Department of Philosophy.

Lack of space prevents me from giving a detailed account of the numerous apparitions seen by Mr. ———. One observation I wish, however, to draw attention to as it is another confirmation of my opinion that ghosts arise only where one is able to project his images. Mr. ——— is able without difficulty to project his apparitions into space but he tells me that the images of his apparitions and the apparitions themselves communicate with him largely through signs. He very rarely hears them speak. Their preference for the gesture language was readily understood when it was found that all of his auditory images were very weak and that he is able only with great difficulty, if at all, to project them into space.

SENSATION AND SYSTEM

By E. B. Titchener

Zeller remarked long ago that "the sole instruments of science are observation and logic," and Armstrong told us the other day that "true science rests wholly upon fact and upon logic."[1] These statements mean, I suppose, that in any system of science as we have it at any particular time two factors are distinguishable: the facts, the data of observation, which are obtained by the methods peculiar to the science in question; and the scaffolding of system, which is obtained by the logical method of reflection. The formulas err, no doubt, on the side of simplicity; but they are right in spirit, even if they need amendment in the letter. A system of science, considered apart from the facts which it embraces, is in reality a complex argument, and like all argument invites discussion.

In a recently published monograph[2] Rahn has passed in critical review certain of the current systems of psychology. He has devoted a good deal of space to an examination of my own attempts to bring our observational data into order. I am interested, of course, in what he has said, and I am grateful for the consideration that he has given me; I only wish that I could now take his criticism, so to say, on its merits, and (so far as I might be able) discuss his objections within a single perspective. That would be the natural procedure; but it is impossible for the reason that, while the monograph is dated December, 1913, the latest of my writings to which it makes reference is the *Text-book* of 1910.[3] There may easily have been circumstances, beyond the writer's control, that caused a delay in publication; but the delay is, so far as I am concerned, doubly unfortunate: first because I have published since 1910 several articles that bear upon questions of system; and secondly because a text-book (and the *Text-book* is most frequently cited) is written for the student and not for the professional psychologist, whereas a scientific article hopes to meet the critical eye. Unless, then, I am to say over again a good deal that has already been said in print, and am to spend time upon changes and corrections that are merely verbal, I

[1] E. Zeller, in Prutz' *Deutsches Museum*, no. 11, 15 März, 1855; H. E. Armstrong, in *Nature*, Oct. 22, 1914, 215.

[2] C. Rahn, The Relation of Sensation to Other Categories in Contemporary Psychology: a Study in the Psychology of Thinking, *Psych. Rev. Mon. Suppl.*, xvi., Decr., 1913, no. 67.

[3] Nothing is said of a delay of publication in the author's Preface, and the list of References (128ff.) gives, curiously enough, only a few scattered dates.

cannot deal with Rahn's commentary as a whole.[4] I regret this limitation all the more since he writes in a sober and objective way.[5]

I begin—to make these remarks as coherent as may be—with a word about the sensation of classification and the sensation of observation. Sensation is a classificatory term. We get our first notion of it, probably, from a common-sense differentiation of the principal organs of sense; sensations come to us from the eye, from the ear, from the skin. We get our systematic notion of it, I believe, as the outcome of abstractive analyses performed under various psychological determinations; in other words,—while the common-sense notion, colored very likely by biology or physiology, is always with us,—we build up the notion of sensation, in a strict procedure, from observations of its empirical aspects or attributes.

Rahn, now, holds the opinion, which to me is strange, that the sensation of psychological classification may be observed (or, at any rate, ought to be observable), introspectively, all at once, in a single observation.[6] In point of fact, however, all experimental investigations of sensation deal with attributes: with qualities, intensities, durations, what not. Even the sensation of Wundt's system, which is constituted solely of quality and intensity, is to be observed under its

[4] Thus, I have discussed the psychological *process* (Rahn, 20-22) in this JOURNAL, xxiii., 1912, 497f.; the introspection of *clearness* (Rahn, 34f.) in *Journ. Phil. Psych. Sci. Meth.*, vii., 1910 (!), 180ff.; the psychology of *meaning* (Rahn, 48) in this JOURNAL, 1912, 165ff.; the psychological *purpose* (Rahn, 65) *ibid.*, 490ff. It is not to be expected that Rahn's criticisms and my own efforts at construction should tally point for point; but my attitude, *e. g.*, to his Section xviii. ("The Differentiation of 'Stimulus' and 'Meaning' Aspects of Percepts and Ideas") may readily be gathered from my paper in this JOURNAL.

[5] On one occasion too soberly! In *Feeling and Attention* (211) I quote Wundt on the distinction of intensity and clearness. "The citation from Wundt is correct; not so the interpretation that Titchener puts upon it. . . . In the very context from which Titchener takes the quotation, Wundt repudiates the interpretation that is put upon it" (Rahn, 17f.). But I supposed that everbody knew Wundt's doctrine; if anyone did not, I had stated it correctly earlier in *Feeling and Attention* (183: Rahn must have missed this passage); I showed by omission-marks that I was quoting only in part; and even so I could not avoid the fatal word *Eindruck*, which is not a Wundtian equivalent for *Empfindung*. What I meant as a mild literary joke—it is somewhat trying to have to explain!—has been taken by Rahn *au grand sérieux*.

There is another criticism which gives me, perhaps, some ground of complaint. I say that "the method of the physical and the psychological sciences is substantially the same" (*Text-book*, 24); and Rahn rebukes me on behalf of feeling (44; cf. 35). But I am not 'hedging' on that score; I expressly deal with the introspection of emotions on 472f.—a passage that Rahn has again missed, though it is separately indexed. I qualify because "the time is not yet ripe for a point-by-point comparison of the methods" (this JOURNAL, xxiii., 1912, 488; cf. the whole section, 485ff.).

[6] *Op. cit.*, 31, 55ff., 76ff., 106ff.

two attributes in separate experiments.[7] But be that as it may, I should certainly maintain in my own case that a sensation, taken in this way as a psychological object of the first order, must ordinarily, by the number and heterogeneity of its attributes, exceed what is called the 'range of attention.' Theoretically, there will be limiting cases: the one, in which the focal attribute is so exceedingly clear that the marginal attributes, correspondingly obscure, are observationally subliminal and touch off no report at all; the other, in which the main levels of consciousness are so nearly at one level that all the attributes touch off reports and all reports are practically alike in fullness of detail. I do not now discuss the question how nearly we are able, in the concrete case, to approach these limits. But I am sure that, in the very large majority of observations, some attribute or attributes will be definitely focal and others as definitely marginal.

It thus appears that Rahn's "anomalous case"[8] is, to my thinking, entirely normal. All observation of psychological objects of the first order is, I conceive, observation of attributes. If, then, we reduce consciousness to a single sensation,—as for the sake of argument we may be allowed to do,—and set to work to 'observe' this 'sensation,' we shall naturally (as I have just said) have some attribute, simple or compound, or some partial group of attributes, as focal, and other attributes as marginal. But this spreading out of a single sensation into coextension with consciousness, for the purposes of observation, does not mean that the sensation of our classificatory list should have two simultaneous clearnesses, a relatively high and a relatively low, or that clearness itself is an attribute of attributes and not an attribute of sensation. I am accustomed to think of the phase of excitation that corresponds with clearness of sensation as the release of a relatively constant amount of energy, the greater part of which flows freely in the course favored by determination, while the remainder flows less freely in the other, associated courses of the excitatory system. That may be mere figure of speech; but it helps us with our present puzzle. For the clearness that attaches to the sensation of our example is the total clearness of the consciousness which that sensation 'fills.' The current doctrine of attention, reinforced by common sense, leads us to emphasize the 'high light' and neglect the 'low light' of consciousness; and yet the total 'attention' that we are 'giving' at any moment is the sum of the two. The fact that every consciousness is patterned into relatively clear and relatively obscure is, to be sure, a fact which demands its own explanation. But it follows from this fact that, if a consciousness is composed of a single elementary process, some of the aspects of the process will be clear and others obscure; which clear and which obscure, is a matter of determination. All the clearness is, in this case, the clearness of the one process (as in more complex cases it is not); and the distribution of clearness, under a general law of consciousness, does not make it into two clearnesses. If, then, we may speak observationally of the 'clearness of a quality' and the

[7] Wundt makes the intensity and quality of pure sensation "jedes ein für sich bestehendes Object psychischer Messung," and bids us, in our search for the mental elements, look for irreducible and invariable 'quality': *Logik*, etc., ii., 2, 1895, 179f., 198. Yet it is the sensation, and not the intensity or quality, that is for Wundt the mental element.

[8] *Op. cit.*, 29.

'obscurity of an intensity' (and we might with the same right speak of the quality of an intensity, or of the quality or intensity of a particular degree of clearness), that manner of speaking does not at all commit us to a classification; the logical status of clearness, in relation to sensation, depends upon our rationalisation of all the observed facts. For, to repeat, the sensation of classification is the logical resultant of many observations. Its qualitative attributes are (in the typical case) points selected by definite procedure from a continuum; its intensive attributes are themselves continua, reduced by the same procedure to a series of points. Hence the conjunction of the qualitative attributes with any points whatsoever upon the correlated intensive scales constitutes, for classification, 'a' sensation; the bracketing together of the qualitative attributes with the complete intensive scales constitutes 'the' sensation.[9] It would be a great simplification of psychology if a sensation, *tota, teres, atque rotunda,* would stand before us under a single comprehensive determination and allow us adequately to observe it as a whole. But that, if it ever happens, happens only after we have made many separate observations of its distinguishable aspects.

Observation is, in fact, conditioned upon two things, peripheral reception and central disposition,—conditioned, as I have put it, upon excitation and determination. We group certain attributes together, in our classification, because, having observed one attribute under a certain determination, we thereafter invariably light upon certain other attributes when, with shift of determination but with excitation unchanged, we undertake to observe them. If, for instance, we have observed a hue, we can always and without exception, under the same conditions of excitation and with fitting determination in each case, observe a tint and a chroma; while we cannot by any kind or force of determination observe a sour or a warm.[10] We regard the attributes as constitutive, again, because the grouping or clustering is thus invariable; a hue that should present itself alone, that should confront us with a blank nothingness when we looked for tint or chroma, is unknown; where there is no observable tint or chroma, there is also no observable hue. In logical terms, the reduction to zero of any member of the attributive group destroys the whole process of sensation. And lastly, we give excitation the logical preference over determination, and take the 'sensation' rather than the 'attribute' as our mental element, because change of determination is limited in its effect to the unravelling of the groups or clusters already referred to; because, in other words, it shows us nothing more than the inseparable aspects of some simplest item of our subject-matter; whereas shift of excitation brings in new matter. We cannot

[9] I am not sure that Rahn is right in denying (as he seems to do: *op. cit.,* 27, 57) the intrinsic difference between qualitative and intensive attributes. In any event, the systematist is justified in making quality the basis of his classification, simply by the fact that qualities have received a goodly number of specific names. It would be absurd to throw away this logical advantage, even if the formal definitions of quality and intensity are (as to my knowledge they are not yet) reducible to identical terms.

[10] I have written elsewhere of psychological determination, and will here simply remind the reader that it need not always be definite, formulable in terms of the known, but may also be indefinite, a determination to look for 'something else of a qualitative sort' or even just to look for 'something different.'

pass, by mere shift of determination, from a hue to a sour; but by mere shift of excitation we may pass from hue to any other sensory quality that there may be. So the chemist (I take an instance at random from Jevons, though Rahn objects to chemical analogies) lists the alkaline metals as elements, and lists as their characters softness, fusibility, volatility at high temperatures, power to combine with oxygen, power to decompose water, and so forth. All analogies halt; but the point here is plain. The chemist, in observations of the first order, will always be experimenting upon softness or fusibility or some such character; and it is not likely that any single experiment will bring out all the characters at once. But he is still experimenting with a metal; the simplest item of his empirical subject-matter, the simplest object or phenomenon of his science, is in this particular chapter of his classification a metal. Hence metals figure as one group of the chemical elements, and the alkaline metals as a sub-group of the metals.[11]

Observation, however, is not confined to objects of the first order. Just as an ornithologist will identify, by species, sex and age, a bird that he has caught a mere glimpse of, on some excursion in the field, so may the psychological observer identify a mental process, elementary or complex, that he has previously made the subject of detailed attributive observation. When an observer who is discriminating tones reports 'kinaesthetic sensations,' the report does not mean that he can give, from his present experience, a precise description, qualitative, intensive, temporal, and so forth, of these sensations; it means simply that he has recognized certain items of experience, immediately, as 'kinaesthetic sensations,' in the way that the ornithologist recognizes his red-eyed vireo. From this point of view, the 'form' of a visual perception may, without any difficulty, become the observed 'content' of an experimental consciousness,[12] or a 'conscious attitude' may be reported as such; only, from the same point of view, we have no psychology of visual perception or of conscious attitude until we have analyzed it into its constituent processes and have brought these simpler processes under the terms of our ultimate classification. The difference between the common-sense and the psychological observation of (for example) visual form is in the first place a difference between the existential determination of science and the interpretative of everyday life, and secondly just this difference of ability or inability to reduce experience to its lowest observational terms.

Rahn makes a great deal, in this connection, of Külpe's experiments on abstraction. Let me remind the reader of the circumstances. In those experiments, a complex visual field was exposed for an eighth of a second. The field comprised four meaningless syllables, which were colored respectively red, green, violet, and black or grey. The syllables were composed each of two consonants and a vowel; no syllable was used twice in the experiment; and the vowels differed at any rate within a given field. The order of the colors, and the arrangement of the syllables themselves, varied from observation to

[11] Rahn, *op. cit.*, 25; W. S. Jevons, *The Principles of Science*, etc., 1900, 675.

[12] *Op. cit.*, 84ff., 89. I need not remind the reader that the nature of the datum of observation, in this instance, is in dispute. I am not here concerned, however, with 'form of combination' in any but an illustrative way.

observation. Five kinds of instruction were employed; the observer was directed (1) to note (*bestimmen*) the total number of visible letters (number), (2) to note the colors and their approximate position in the field (color), (3) to note the form which the syllables outlined (form), (4) to note as many individual letters as possible with their approximate position (elements), or (5) simply to observe. Külpe found—I take his extreme case—that under the instruction for form no report at all might be obtainable of color or elements. "The observers believed . . . that they had not, as a matter of fact, perceived any color, any object, etc." From results of this sort far-reaching conclusions are drawn—as regards sensation! "Since psychology as science ascribes uniformly determinate attributes to sensasations, regards them as constituted of determinate partial contents, it follows that psychology distinguishes between psychical processes and the consciousness of such processes."

Here, then, is a highly complex visual 'stimulus,' presented tachistoscopically, and observed under cognitive instruction. For number, form, position and letter-element, whatever they may be, are certainly not sensations; and though the observers may have shifted to an existential attitude under the instruction for color, the probability is that they did not. Suppose, now, that under the instruction for form an observer has reported 'diamond,' and that he is able to say nothing when questioned regarding color and elements. What precisely, in the case of color, does this 'nothing' mean? It is hardly likely that the form was so bare, so disembodied, that it might have been, indifferently, a visual or a tactual form. We assume that it was perceived as a visual form, and might have been so reported. But then is it not reasonable to suppose, further, that it could have been reported as dark on light, or darker on lighter? For the stimulus is always projected upon the bright disc of the lantern-screen; a form that is made out at all must be made out upon this light background; can it be imagined that an observer, under the instruction for form, would be wholly unable to say whether the form glowed with a brighter light or stood out as darker upon the light surface? But dark and light are qualitative terms, and Külpe's 'color' included black and grey. Let us turn, however, to the letter-elements. "The observer may be able correctly to describe a *form*, without having experienced anything whatever directly in *consciousness* in regard to the nature (*Beschaffenheit*) of the objects which delimited it." What again, precisely, does the 'nothing whatever' mean? Under the instruction for elements, the observer is to note as many individual letters as possible. Does the 'nothing whatever' simply mean, then, that the observer could not say whether the form was bounded by individual letters? that he could not identify any part of the boundary as a letter? Apparently. But he might still have been able to report whether the boundary was continuous or discrete, mottled or uniform, broad or narrow, straight or wavy; more especially, one would think, since the report of contents not favored by instruction was given "vielfach nur auf Grund von Reproduktionen;" and adjectives of this kind do, in a way and to a degree, characterize the limiting objects.

Külpe's account is tantalizingly brief. But we get an idea of the complexity of the field from the remark of one observer that he had frequently seen (or been conscious of) more than he was afterwards able to report. We get an idea of the cognitive emphasis in the instruction from the remark of another observer that the process of

noting or specifying (*bestimmen*) seemed to be entirely separate from that of seeing. Külpe's own inference that the changes wrought by instruction are changes of *Auffassung* and not of *Empfindung* seems imperilled by his later statement that "die Versuchspersonen glaubten tatsächlich die Endrücke in der angegebenen Unbestimmtheit zu *sehen*" (italics in original). But indeed the word *Empfindung* is sadly out of place in this context. Here are apprehension, identification, meaning, perception, reproduction; here, in other words, is observation solely of the higher orders. To argue from the introspective reports—by way of sensation!—to a rehabilitation of the 'inner sense,' and a distinction between conscious actuality and psychical reality, and a furtherance of the old dispute between nominalism and realism, is to erect a heavy superstructure of system upon curiously insufficient observational foundations.[13]

What it is, in observations of a higher order, that touches off the introspective report is a matter to be discovered by experiment in the particular case. I have not repeated Külpe's experiments, and therefore cannot—in this particular case—be specific.[14] But I can now make a general statement which may be to Rahn's point. I spoke above of limiting cases in the observation of the sensation-consciousness, and I left them without discussion (p. 260). But I have no hesitation in saying that the realisation of the first limiting case has no terrors for me; and that certainly I should not be driven by it to a distinction of conscious actuality and psychical reality. There is no difficulty in conceiving an observer to be so completely under the influence of determination that the marginal attributes are too obscure to connect at all with the mechanism of report. Külpe remarks, and correctly, that "die Lücken im Protokoll erlauben nicht ohne weiteres den Schluss auf eine entsprechende Lücke im Bewusstsein der Versuchspersonen;" and it seems fair to suppose in our imaginary instance—just because that instance comes to us as a limiting case—that the margin was still 'there,' in consciousness, although it was so obscurely 'there' that it failed to touch off a verbal description. Might we not, from what we know of the pattern of consciousness, even predict, before the event, that once in a great while such cases will occur?

But the disagreement between Rahn and myself goes deeper, I am afraid, than this matter of classification, and of the relation of scientific system to scientific data. I cannot help thinking that he

[13] O. Külpe, *Versuche über Abstraktion*, Bericht über den I. Kongress für experimentelle Psychologie in Giessen, 1904, 56ff. Rahn even translates *Beschaffenheit* (of the letter-element) by 'attributes,' as if the letter-element were a sensation: *op. cit.*, 78.

[14] The instruction, as I have said, was cognitive; the observer was asked to note or determine or specify (*bestimmen*). It is fairly certain that, under this cognitive determination, something in the visual field, some phase of the complex 'stimulus,' actualized the 'meaning' of the instruction, which then touched off the report. The focus of consciousness would thus be held by processes correlated with the stimulus-phase and the meaning. It would probably not be easy, in the circumstances, to indicate all these processes; and it would probably be impossible to name their relevant attributes, and so to reduce the observation outright to that of an object of the first order. Külpe says (*op. cit.*, 66): "Die Durchführung der Aufgaben ist nicht näher zu beschreiben," 'cannot be described in detail.'

is still, as regards the psychology of sensation, held in the trammels of the older associationism. It would indeed be surprising, were it not so common, to note how the critic of current sensationalism persists in ascribing to sensation, almost as if by instinct, the characters which the associationist school attributed to idea. In the typical associationist system the idea was, first and foremost, a meaning; as a meaning, it was stable and permanent; and its association to other and similar ideas gave rise to all kinds of mental formation. We may insist, now, that meaning has been pared from sensation,—we may insist that meaning, from the psychological point of view, is in general something detachable, something that accrues, something variable and incidental; yet we cannot drive that point home; our critics still write as if sensation involved awareness. We may insist, again, that sensation is a process; we may give illustration, and try in all manner of ways to make clear what, in the psychological sense, a process is; we are told that the position is untenable and impossible, and that our sensory element must in reality be something fixed and static. We may insist that sensation is an analytical and not a genetic concept; and we are told, notwithstanding, that it is generative: not any more, perhaps, that it generates our perceptions,—that mistake one may hope never again to meet in such form that it need be taken seriously; but certainly that it ought to generate better than it does, that it is inadequate to our complexes. I say 'we insist,' because I am here outlining a position in dogmatic terms; in fact, of course, we argue from such evidence as we can bring forward. But the arguments and the evidence seem hardly to count; it is the position itself, if I am not mistaken, that is repugnant, that is instinctively rejected; a consideration of arguments and evidence—as distinct from a mere confrontation of system with system—would be very welcome. And the reason for the repugnance seems to lie in the carrying over into sensationalism of the doctrines of associationism.

Thus, at the very beginning of his enquiry, Rahn refers to "the structuralistic doctrine that under certain conditions we may become aware of redness and blueness."[15] At first thought one would say that this is a common-sense rather than a psychological doctrine. But the context shows that Rahn looks upon 'awareness of redness and blueness' as a paraphrase of the structural term 'color sensation.' As well might one declare that 'awareness of fusibility' is a particular chemical element! The phrase is, indeed, quite foreign to the structuralist's ways of thinking. He can 'observe a red,' and he can—like everybody else—'apprehend a meaning;' observation he will deal with under the heading of Method, and meaning he will deal with, from the point of view of analytical psychology, under its own heading. But he has nothing more to do with 'awareness' than has the chemist or the physicist or the biologist; and yet Rahn, without hesitation and in good faith, introduces the term into a passage that refers to structuralist doctrine! With this prepossession upon him, how shall the critic come to close quarters with the system he is criticizing?

As with meaning, so with process: "if the element is to be considered as a part, an irreducible ultimate content of consciousness, then it cannot at the same time be regarded as a process, for a

[15] *Op cit.*, 3; cf. 60, 62, 78, etc. I do not see how the charges brought against me on 40, 94, are to be squared with the sentence from which I quote; but they are themselves met, I hope, by *Thought-processes* 60f. and *Text-book* 545.

process implies the interrelation of a number of elements or factors;" and so the poor element, in spite of protest, must remain "static."[16] Naturally, if sensation is a meaning and consciousness is awareness! But might it not have occurred to Rahn that things are less simple than this? that, if his process is the resultant of several elements and mine requires but a single element, we may perhaps not define 'process' in the same way? And might it not have been both more effective and more profitable to understand my 'process' and wrestle with that than to substitute another kind of process for it?

Here are two of our associationist parallels, plainly drawn. As for the third, Rahn is clear of the cruder form of what I must call—from my own standpoint—the genetic fallacy; he by no means escapes it altogether. The "kinaesthetic sensations that attention lights upon, when it pounces upon such a subtle psychosis as a state of doubt, are not the psychic equivalents of the state itself."[17] But, it seems to me, the task of psychology, over against the 'I doubt' which expresses some common-sense or logical determination of the organism, is precisely the task of analysis; and if analysis brings to light nothing else than kinaesthetic sensations, then these sensations (characterised as accurately as may be) are precisely the 'psychic equivalents' of the logical situation. Description of process is thus correlated with statement of meaning. If Rahn now replies—as he probably would—that innumerable analyses by the most competent observers would still fail to put him into what he regards as psychological possession of 'so subtle a psychosis,' I must repeat what I have said before: first, that a description, as compared with an intimation of meaning, is always clumsy and long-winded; but, secondly, that psychology—as I regard it—"is under no obligation to reconstitute, at the end of an investigation, the [continuum of] psychical experience; it is enough to have shown that its contributions to the analysis of that experience are valid."[18] In other words, as I have also said elsewhere, a descriptive science may not justly be asked to give more than, by its nature as descriptive science, it can properly furnish.

James Mill, although he tries hard to make his readers get the qualitative 'feels' of the various sensations, can nevertheless speak of sensations of resistance, of hardness, of weight, of shape, of size, because the sensations are, for him, "the feelings from which we derive our notions of what we denominate the external world;" "it is acknowledged on all hands that we know nothing of objects, but the sensations we have from them;" "in using the names tree, horse, man, the names of what I call objects, I am referring, and can be referring, only to my own sensations."[19] Mill, therefore, is perfectly logical in regarding a process as an affair of "certain steps," and in declaring that consciousness of change is "an essential part of the process" of sensation: "having *two* sensations is not only having sensation, but the only thing which can, in strictness, be called having sensation."[20] And Mill can undertake to resolve doubt—"a

[16] *Op. cit.*, 21f., 67.

[17] *Op. cit.*, 45.

[18] This JOURNAL, xxiii., 1912, 166; *Thought-processes*, 1909, 284f.

[19] *Analysis of the Phenomena of the Human Mind, i.*, [1820] 1869, 58, 77, 79; 3; 349f.; 93; etc., etc. I refer to sensation, rather than to the more obvious idea, in order to bring out clearly the resemblance between Rahn and a representative member of the associationist school.

[20] *Op. cit.*, ii., 13f.; 11f. The whole section is significant.

phenomenon of some complexity, but of which the elements are not very difficult to trace"[21]—into 'psychic equivalents' that shall leave no remainder, because he is translating common sense into associationist terms and offering the result as psychological analysis. All this is intelligible enough; but it is not structural psychology.

There will probably be no end to the rivalry of psychological systems. A science is never finished; and as the data of observation multiply, the systematic argument must be changed and extended to cover them. That apart, psychology in hardly yet free from its traditional connection with philosophy, and psychologists are accordingly not yet at one even as regards the general type to which a system must conform. No more can be said, therefore, than that here, as everywhere, the system is subordinate to the data, and reflection secondary to observation. But it is well to keep this principle steadily in mind: for only in its light shall we apply the right test to our systems, the test of their ability to organise, consistently and exhaustively, such data as we have. If we thus become a little mistrustful of programmes, is there any harm?

[21] *Op. cit.*, i., 270, 430.

SOUND LOCALIZATION UNDER DETERMINED EXPECTATION[1]

By L. R. Geissler

It had frequently been observed by the writer that under ordinary circumstances his sound-localization seemed to be strongly influenced by a determined expectation or direction of attention. The only direct reference to this problem in the experimental literature known to the author is that made by Pierce,[2] in connection with experiments in which "the method of procedure was to move the sound until the subject was aware of the direction in which movement was taking place." In discussing the results, Pierce says: "from among the various directions theoretically possible from a physical point of view, certain circumstances will usually determine which direction shall be actually perceived. The expectation of the subject is often the deciding factor. This expectation may be the result of a recent localization which makes one direction of the attention easier than another, or it may be due to some suggestion inadvertently given. And when once a given direction of movement has been perceived, false though it may be, localizations of adjacent positions will the more readily fall in line with the direction first perceived. This determining influence of both suggestion and of an antecedent localization will appear still more clearly on later pages." Our present experiments were planned for the special purpose of determining quantitatively to what extent the expectation of a stationary sound from a certain direction influenced the localization of this sound.

The writer was greatly aided in the conduct of these experiments by Mrs. Ruth Collins Desch, who had previously in co-operation with C. E. Ferree investigated another phase of the problem of auditory localization.[3] Our experiments began Jan. 24th and lasted to April 25th, 1914. They were performed on twenty observers, each of whom made 68 individual localizations which could be finished in a single session of not more than about 90 minutes. Twelve observers were men students of the senior class who had completed a full year's introductory course (based on Titchener's "Textbook"), and about four months or more of qualitative laboratory work (based on Titchener's Qualitative Students' Manual), which had included in every case the experiment on touch-localization (No. 33) as a sort of special preliminary training. The eight women observers ranked from the junior class of a girls' preparatory school to teachers of several years' experience in that school; none of them had had special

[1] From the Psychological Laboratory of the University of Georgia, Athens, Ga. Read in parts before the Am. Psych. Association, Dec. 31, 1914, Philadelphia, Pa.

[2] A. H. Pierce, Studies in Auditory and Visual Space Perception, 1901, 57.

[3] C. E. Ferree and Ruth Collins, An Experimental Demonstration of the Binaural Ratio as a Factor in Auditory Localization, Am. J. Ps., April 1911, xxii, 250-297.

psychological preparation. The writer gladly takes this opportunity to express his hearty thanks to Mrs. Desch for many helpful suggestions in performing the experiments and in preparing this article, as well as to Misses M. E. Jeffcott, Dorothy Selby, Sara Lamar, Helen Michael, Janet Newton, Pauline Odum, Julia Orr, Ruth Reid, and to Messrs. G. Barrett, A. B. Bernd, T. N. Hendricks, W. E. Hitchcock, F. Holden, T. A. Maxwell, J. E. McDonald, L. Michael, J. Myers, H. M. Pitts, E. R. Pund, and H. D. Williams, for their patient and conscientious work as observers and their deep interest in the problem.

The sound-cage was built in the middle of a symmetrically constructed room of 3.1 x 6.2 meters, which contained only one table and two chairs, so that possible echoes from the faint stimulus were neutralized or, more probably, eliminated. The observer's head, facing the windows, was in the center of a noiselessly rotating wooden lever-system which carried two little telephone receivers at exactly opposite positions, about 75 cm. from the center of the head, and at the level of the ears when the observer sat on a chair placed in the center of the apparatus. The sound-stimulus was produced by noiselessly and quickly closing and opening a double-throw baby-knife switch in circuit with the receiver and two dry cells. The observers were blindfolded, care being taken that the bandage did not touch the pinna. The position of the stimulus was determined by means of a protractor-scale in the center of the lever-system above the observer's head. The zero-point was at the back, 180 in front, the positions on the right were called *plus*, those on the left *minus*, so that the aural axis passed through $\pm$ 90°. While the sound could be given at any point on the circumference of the horizontal plane passing through the ears, the observers were instructed that it would be given only at 10° intervals; actually it was given only at intervals of 30°, namely 0, $\pm$ 30, $\pm$ 60, $\pm$ 90, $\pm$ 120, $\pm$ 150, and 180. The observers were given a sufficient number of preliminary trials to memorize this schema. The localizations were made in two ways, by pointing with finger and by visual-verbal reference to the scale, unless the latter method was preferred exclusively. The bandaged eyes prevented the use of the cardboard chart employed by some investigators. Whenever an observer used both methods, care was taken that pointing and verbal reference to scale did not conflict.

In our method the greatest importance must be laid on the instructions given to the observers and the subsequent reports demanded from them. After having been told that the general purpose of the investigation was to find out how well they could localize a faint sound under certain circumstances, they were instructed sometimes to direct their best attention to one part of the circle only, to think of it and nothing else, and to expect the sound to come from that particular direction. If sometimes the sound seemed to come from a place entirely different from the one indicated, that fact should not in any way disturb their future observations, as such differences were naturally to be expected. Again, they were told that sometimes they were not to limit their attention and expectation, not to expect the sound from any definite point; this instruction was briefly expressed by the command "Unlimited." Then, as soon as the sound was heard, its position was to be indicated immediately, and in addition the observers were asked to tell from what point they had expected the sound, either definitely or in general, that is, whether from points or from areas or regions on the scale. They were urged to be perfectly frank to tell whether they had disobeyed the instruction or not, as that mistake was less serious than deception. Aside from the

behavior toward the instruction "Unlimited," which will be dealt with in detail under the rubric of results, several observers reported failures to follow the instruction for special direction of expectation in certain cases, in which the experiments were repeated at a later time. Both reports of localization and expectation were recorded, and the writer is confident that instructions were conscientiously carried out by all observers. In addition to these reports, ten of our men observers were also asked at the end of their respective sessions to give an introspective account of the usual course of consciousness during an observation, beginning with the instruction and stopping with the beginning of the report.

The 68 observations made by each subject were divided into two groups, the first containing 5 series of ten tests each. A short pause was made after each of these series and a longer one after the whole group. The 18 observations of the second group were taken without any interruptions. In the first group, before each observation, the subject was instructed whether to limit his expectation of the sound to one of the halves, by the words "right half," "left half," "front half," or "back half," or whether not to expect the sound from anywhere in particular, by the word "unlimited." A second after this instruction the warning signal "ready" was given, and after about two more seconds the sound-stimulus was presented. The positions 0 and 180 were never used with the instruction "right" or "left;" likewise ± 90 were omitted as stimuli when expectation was to be limited to the front or back half. Each of the 12 stimulus-positions was presented once with the instruction "unlimited." With limited expectation the stimulus was presented once at each position in the *required* half and once at each position in the *opposite* half. The intermediate positions ± 30, ± 60, ± 120, and ± 150 had thus to occur four times each, while the corner positions ± 90, 0, and 180 occurred only twice each, in addition to the single occurrence with "unlimited" as instruction. Each series of ten observations included two "unlimited" cases and two cases for each half, in such a way that the stimulus was actually given in the *same* half as often as in the *opposite* half. In the second group of experiments the expectation was limited to the quadrants by the instructions "right front," "left front," "right back," and "left back." Only the intermediate positions were used as stimuli, except in one "unlimited" case and in another case where the sound was presented in some "different" position; otherwise these experiments were carried on like those of the first group. The following schema represents the actual order of instructions and corresponding stimulus-positions which each of the twenty observers completed.

SCHEMA OF ORDER OF PROCEDURE

Instr.	Stim. Pos.	Ser. 1	Ser. 2	Ser. 3	Ser. 4	Ser. 5	Instr.	Stim. Pos.	Quadrants	
Unlim't'd	anywhere	+120	+90	+30	+150	+60	Rt. Fr.	same Qu.	+150	+120
Fr. half	same half	−120	180	−150	+120	+150	L. B'k.	diff. "	+30	+150
Rt. "	opp. "	−90	−150	−120	−30	−60	L. Fr.	same "	−120	−150
L. "	same "	−30	−60	−90	−120	−150	Rt. B'k.	diff. "	180	−120
B'k. "	opp. "	180	+150	+120	−150	−120	Unlim.	anywh.	−60	0
Rt. "	same "	+150	+120	+90	+60	+30	L. B'k.	same Qu.	−30	−60
Unlim't'd	anywhere	−150	−120	−30	180	−90	Rt. Fr.	diff "	−150	−30
Fr. half	opp. half	+30	+60	0	−60	−30	Rt. B'k.	same "	+60	+30
B'k. "	same "	−60	−30	+60	+30	0	L. Fr.	diff. "	+120	+60
L. "	opp. "	+60	+30	+150	−90	+120				

A brief examination of this schema will show that the same combination of instruction and stimulus-position was never given more than once in each series.

In computing and tabulating our results we fractionated the data both as to stimulus-position and as to instruction concerning the direction of expectation, and averaged either localizations of all twenty observers or of one group of observations as compared with another group. The percentages of error in localization for any given direction of expectation with all possible stimulus-positions were calculated by adding the errors, that is, the differences between the stimulus-positions and the corresponding averages, and dividing this sum by the sum of all employed stimulus-positions. Conversely, the magnitude of error, or amount of displacement, for any given stimulus-position under all kinds of expectation employed was determined by adding the corresponding averages, dividing the sum by the number of cases involved, and subtracting the result from the stimulus-position. In the Tables which follow the decimal fractions have been omitted, although at least one decimal position was always used in the calculations. Among the various abbreviations used in the Tables, No. refers always to the number of cases from which the average or error is calculated. MV. stands for mean-variation, E. means error, and Tot. or T. stands for total; other abbreviations will be self-evident or explained in the subsequent discussion of the Table in which they are used.

Before attempting to determine whether direction of expectation has any influence upon sound-localization, it is necessary to study our data from a general standpoint and as far as possible to compare them with results obtained by previous workers in this field. For this purpose we must refer the reader to rubric h of Table I and to the first 28 rows of Table II. These results show clearly the general tendency of sound-displacement that other authors have found, namely that sounds from behind the ears are frequently referred forward and those from in front are referred backward, that is, are referred toward the aural axis or in some cases even beyond it. In our results the greatest displacement occurs with the stimulus at 0, the sixty localizations averaging 129.2; but if we subtract from these the twenty localizations under instructed forward direction of expectation, the remaining forty observations average still as high as 123.2. The actual places at which stimulus 0 was localized are shown in rows 1 and 2 of Table II. In row 2, for example, that is with "unlimited" and "back half" direction of expectation, the sound coming from 0 was localized 9 times at 0, twice at 10, once at 120, 3 times at 150, and so forth. Likewise all the other even-numbered rows contain the reduced observations, that is, those localizations in which the direction of expectation was to the same half in which the sound would be given. With the exception of stimulus-positions ± 90 the "reduced" observations show a smaller displacement error than the total, thus indicating in general the unfavorable influence of direction of expectation upon the accuracy of sound-localization. With the stimulus at 180 our results show a good deal less backward displacement than other investigators seem to have observed, the error being only 10.5 for the reduced number of observations. The smallest errors occur with stimuli 90, which show a slight backward displacement of about 5°. The second largest error is found with 30, a displacement of 75.2° and 67.5° for the right and left side respectively, while all other stimuli show an error varying between 19.0° and 25.0°, either forward or backward.

TABLE I

Rows	Rubrics	a			b			c			d			e		
	Instr. Stimulus Position	Unlimited or No Expectation									Expectation Limited to Right or Left					
		Gen. Results			No Exp'n.			Vol. Lim. Exp'n.			St. in Opp. H.			St. in Same H.		
		Av.	Mv.	E.	No.	Av.	Mv.	No.	Av.	Mv.	Av.	Mv.	E.	Av.	Mv.	E.
1	0	135	54	135	3	120	40	17	138	49	..	..	..	..	..	..
2	+30	120	23	90	7	121	30	13	119	20	109	37	79	119	26	89
3	−30	116	53	86	5	66	47	15	133	32	104	38	74	102	54	72
4	+60	87	15	27	6	108	18	14	81	14	84	14	24	92	8	32
5	−60	91	16	31	7	86	13	13	98	11	88	14	28	85	20	25
6	Tot. Bck.	110	..	74	28	100	..	72	114	..	94	..	51	100	..	55
7	+90	86	12	4	6	95	13	14	83	10	91	5	1	82	8	8
8	−90	90	13	0	5	84	9	15	93	15	88	12	2	89	12	1
9	+120	91	12	29	9	93	9	11	90	14	95	15	25	95	22	25
10	−120	107	15	13	8	109	21	12	106	12	99	10	21	91	8	21
11	+150	130	16	20	5	126	19	15	131	20	137	25	13	115	23	35
12	−150	139	13	11	8	120	24	12	152	19	142	12	8	125	23	25
13	180	151	29	29	6	142	57	14	155	39	..	..	..	..	..	..
14	Tot. Fr.	124	..	20	36	119	..	64	127	..	118	..	17	107	..	28
15	No. Expt.	240			75			165			200			200		
16	$\frac{\Sigma \text{Errors}}{\Sigma \text{St. Pos.}}$	$\frac{475}{1080}$ = 44.0%			$\frac{462}{1080}$ = 42.8%			$\frac{487}{1080}$ = 45.2%			$\frac{275}{900}$ = 30.0%			$\frac{331}{900}$ = 36.8%		
17	Eliminating 0, ±90, and 180	$\frac{307}{720}$ = 42.6%			$\frac{293}{720}$ = 40.7%			$\frac{316}{720}$ = 44.0%			$\frac{272}{720}$ = 37.8%			$\frac{323}{720}$ = 44.9%		

TABLE I—*Continued*

Rows	Rubrics	f			g			h			i			j		
	Instr. Stimulus Position	Expectation Lim. to Front or Back						Total Results Rubrics a and d–g			Expectation Lim. to Quadrants					
		In opp. half			In same half						St. in diff.			In same Qu.		
		Av.	Mv.	E.	Av.	Mv.	E.	No.	Av.	E.	Av.	Mv.	E.	Av.	Mv.	E.
1	0	141	56	141	110	75	110	60	129	129	..	..	..	..	..	..
2	+30	126	88	96	76	44	46	100	109	79	110	44	80	85	33	55
3	—30	117	46	87	74	53	44	100	103	73	126	32	96	85	45	55
4	+60	99	13	39	74	12	44	100	87	27	98	15	38	66	14	6
5	—60	93	11	33	76	18	16	100	87	27	Not given			72	14	12
6	Tot. Back	115	..	79	82	..	46	460	103	67	111	..	71	77	..	32
7	+90	..	..	..	..	..	..	60	86	4	..	..	..	..	..	..
8	—90	..	..	..	..	..	..	60	86	4	..	..	..	..	..	..
9	+120	82	22	38	104	10	16	100	94	26	103	11	17	102	11	18
10	—120	87	11	33	99	10	21	100	97	23	102	12	18	96	12	24
11	+150	113	37	37	126	20	24	100	123	27	138	18	12	140	12	10
12	—150	100	43	50	139	16	11	100	129	21	140	11	10	132	14	18
13	180	106	81	74	167	26	13	60	142	38	160	15	20	..	..	..
14	Tot. Front	98	..	46	127	..	17	460	115	29	129	..	15	118	..	20
15	No. Expt.	200			200			1040			160			160		
16	ΣErrors / ΣSt. Pos.	630 / 900 = 70.0%			294 / 900 = 32.7%			570 / 1080 = 47.0%			290 / 840 = 36.0%			208 / 720 = 29.0%		
17	Eliminating 0, ±90, and 180	415 / 720 = 57.6%			171 / 720 = 23.8%			295 / 720 = 40.1%			270 / 660 = 40.9%			208 / 720 = 29.0%		

TABLE II

	Possible Positions:		0	10	20	30	40	50	60	70	80	90	100	110	120	130	140	150	160	170	180	Tot. No.	Av.	Error
1	Tot.	0	13	2	..	..	..	..	..	..	..	..	..	..	1	..	..	3	6	10	25	60	129.2	129.2
2	Red.		9	2	..	..	..	..	..	..	..	..	..	..	1	..	..	3	5	6	14	40	123.2	123.2
3	Tot.	+30	..	1	4	2%	6	2	5	2	3	7	8	6	17	8	5	9	7	4	4	100	109.1	79.1
4	Red.		..	1	3	2	3	..	5	..	2	4	6	3	9	6	4	7	2	2	1	60	105.2	75.2
5	Tot.	−30	4	4	1	7%	2	8	2	2	2	3	3	3	10	3	1	9	16	8	6	100	102.7	72.7
6	Red.		4	3	5	4	1	5	2	1	1	1	1	1	6	1	..	6	10	5	3	60	97.5	67.5
7	Tot.	+60	1	..	..	..	2	2	11%	5	12	41	14	4	5	2	..	1	..	..	..	100	86.9	26.9
8	Red.		1	..	..	..	1	1	8	5	9	23	7	3	1	..	..	1	..	..	..	60	83.5	23.5
9	Tot.	−60	..	1	..	1	2	1	11%	12	8	39	9	6	3	3	3	1	..	..	..	100	86.9	26.9
10	Red.		..	..	..	1	2	1	9	9	5	21	3	2	1	2	3	1	..	..	..	60	84.8	24.8
11	Tot.	+90	..	..	..	..	1	..	5	2	12	31	6	3	..	..	..	..	..	..	..	60	86.0	4.0
12	Red.		..	..	..	..	1	..	4	2	8	20	3	2	..	..	..	..	..	..	1	40	84.5	5.5
13	Tot.	−90	..	..	1	..	..	2	3	4	7	29	6	3	3	1	..	..	..	..	1	60	86.1	3.9
14	Red.		..	..	..	..	..	1	3	4	5	18	4	1	2	1	..	..	..	..	1	40	85.2	4.8
15	Tot.	+120	1	..	..	4	1	1	1	2	9	33	22	14	8%	2	1	..	1	..	..	100	93.6	26.4
16	Red.		..	..	..	1	1	..	..	2	4	20	15	10	6	1	1	..	..	..	..	60	96.7	23.3
17	Tot.	−120	..	..	..	1	..	..	..	5	7	41	22	15	5%	1	1	1	..	..	1	100	96.8	23.2
18	Red.		..	..	..	..	..	..	..	2	6	20	15	10	4	1	1	..	..	..	1	60	99.2	20.8
19	Tot.	+150	1	1	..	2	1	3	..	2	3	3	6	8	18	11	5	19%	14	2	1	100	122.9	27.1
20	Red.		..	1	..	1	..	2	..	..	1	1	3	7	14	6	3	13	8	..	..	60	125.0	25.0
21	Tot.	−150	1	2	..	2	..	1	..	1	1	4	7	2	14	17	15	15%	13	5	..	100	129.3	20.7
22	Red.		..	1	..	..	..	..	..	..	1	2	6	2	9	9	12	7	8	3	..	60	131.0	19.0
23	Tot.	180	6	6	..	..	..	..	..	..	..	..	..	..	..	..	..	2	2	10	34	60	141.7	38.3
24	Red.		2	2	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	9	26	40	169.5	10.5
25	Tot. No.		27	17	12	19	15	20	38	37	64	231	103	64	84	48	31	60	59	39	72	1040	Rows 1–28 show Frequency of S'd Localization at Possible Positions	
26	Grouped		27	222								231	488								72	1040		
27	Red. No.		16	10	8	9	9	10	31	25	42	130	63	41	52	27	24	39	33	25	46	640		
28	Grouped		16	144								130	304								46	640		
29	Unlimited		10	1	..	2	1	..	2	3	2	28	3	6	10	7	12	17	17	9	35	165	Rows 29–34 show Frequency of Expectation of the Sound from Possible Positions	
30	Expectations		10	11								28	81								35	165		
31	Limited		65	16	19	42	22	18	29	14	22	138	17	20	47	18	21	31	39	21	56	655		
32	Expectations		65	182								138	214								56	655		
33	Total Expectations		75	17	19	44	23	18	31	17	24	166	20	26	57	25	33	48	56	30	91	820		
34	Grouped		75	193								166	295								91	820		

In general these results agree, as far as agreement may be expected under our peculiar conditions, with those of previous investigators, with the one slight exception that our data seem to emphasize the tendency of sound-displacement toward the aural axis more than any other observer has so far found.

The next question, then, is to find out whether and to what extent this general tendency is modified by the determination to expect the sound from certain regions of the circumference. For this purpose we may best begin the fractionation of our data with the cases under the instruction "unlimited," because here we have a contrast between observations in which this instruction has been successfully carried out and cases in which the observers of their own accord, whether on account of misunderstanding the instruction or for other reasons, voluntarily limited their expectant attention either to definite points or to certain narrow regions of the scale. The frequency and direction of the voluntary and definitely limited expectations, regardless of individual observers, may be seen in rows 29 and 30 of Table II, while in Table III the frequency distribution of these cases is given with regard to the observers, but without reference to the direction of the voluntarily limited expectation. It will be seen from both Tables that out of the 240 possibilities, 165, or 68.75%, of the cases show the definite voluntary restriction of expectation. This tendency seems to have had no decided influence upon the accuracy of localization, as a comparison with the indefinitely restricted and the entirely unrestricted expectations indicates, according to rubrics b and c in Table I. In 71 of the 165 cases it accidentally happened that the voluntarily selected point of expectation occurred in the same quadrant as the stimulus-position. But in spite of this agreement, the accuracy of localization in these 71 cases was 3.5% less than in the remaining 94 cases. A similar difference is found between rubrics b and c, the voluntary definite restriction giving less accurate results by 3.6% than the entirely and the partly indefinite expectation. If we now compare the combined or "General Results" under "unlimited" instruction, as given in rubric a, with the total results of rubric h, the former seem to be by 3% more accurate than the total. The latter, however, is exaggerated on account of rubric f alone, which presents a special condition to be considered separately later. Without rubric f the total error would be only about 35%, which compared with the errors of rubrics a, b, and c is considerably less. In other words, the instruction "unlimited," whether faithfully carried out or not, interferes decidedly with the accuracy of localization. The introspections offer, as an explanation of this fact, unsteady attention and especially moving visual images. In addition there seems to be a tendency among some of the observers to waver between attempts not to limit expectation and temptations to visualize certain portions of the scale. On the whole the quantitative and introspective data of these experiments under "unlimited" instruction furnish an unlooked-for confirmation of the writer's previous investigation of consciousness under negative instruction.[4]

Considering next the actual direction of expectant attention, when it is thus voluntarily limited to certain points, we find, according to rows 29 and 30 in Table II, that out of 165 cases it was limited in 116 cases to the front half exclusive of ± 90; if the latter be included,

[4] L. R. Geissler, Analysis of Consciousness under Negative Instruction, Am. J. Ps., April, 1912, xxiii, 183-213.

the figure rises to 144. That is to say, only in one out of eight chances do we ordinarily expect a sound to come from behind the ears. A similar tendency is evident in the experiments with limited expectation, where according to instructions all quadrants were equally often represented. Nevertheless the expectations from the front, exclusive of ± 90, are by 10% more numerous than those from the back, according to rows 31 and 32 of Table II. The reason for this preponderance of the front half lies probably in the importance of vision for our daily life,—"a sort of racial habit," as one of our observers called it in his introspections. Among the positions in the front, those of the right quadrant are preferred over those of the left in the ratio 4:3, perhaps because of the righthandedness of our observers. Taken all together, about 50% of the voluntary restrictions of expectation refer to the right front quadrant, about 37½% to the left front quadrant, and only about 12½% to the back half. To summarize, the negative instruction is in a large number of cases understood in the positive sense of "expectation *ad libitum*." With regard to the influence of expectation, this set of results has been neither definite nor extensive enough to indicate any regularities.

The experiments with positive instruction for definite restrictions of expectation, however, show undisputable evidences of the extent to which expectation may influence the accuracy of sound-localization. As should be expected, our results have not shown a single case in which a stimulus on the right or left has been localized on the opposite side. All confusions or displacements of sound occurred between the front and back halves. The most accurate localizations in this set of experiments seem to have been made when expectation was directed to the right or left half opposite to the actual stimulus-position; *cf.* rubric d in Table I; while one would have expected the best results when expectation was directed to the same half, that is, under rubrics e and g. This result is, however, somewhat distorted on account of the presence of data with the extreme stimuli 0 and 180 and the absence of data with stimulation ± 90 in rubrics f and g. Eliminating the results of rows 1 and 13 in rubrics f and g and those of rows 7 and 8 in rubrics d and e, we obtain the corrected results of row 17. But even so rubric d offers the second lowest percentage, while under rubric e the results still show a more marked effect of displacement toward the aural axis than under d or g. The only explanation that we can offer for the difference between d and e,—that is, for the fact that expectation to the opposite right or left half to some extent diminishes the earward displacement and the resulting inaccuracy of sound-localization,—consists in the assumption that such a direction of expectation does away with the kinaesthetic and visual ear-consciousness that we frequently experience in listening to faint sounds and that was explicitly mentioned in the introspections of one of our observers. This explanation is supported by the outcome of the second group of experiments, rubrics i and j, as in these cases the direction of expectation to the quarters did not favor the positions 0, 180, and especially ± 90, and therefore was rather applied to the middle of the quarters. Consequently, when the sound was given in the same quadrant to which expectation was directed, the errors were very small, averaging only 29%, while in case the stimulus was in some quadrant different from the one expected, the percentage of error was 36.0; with elimination of the one case of 180 (row 13, rubric i), it is 40.9%. This result is almost the same as that of rubric b, where there was practically no expec-

tation, and where therefore the usual forward and backward confusion observed by practically all our predecessors was influenced least, if at all, by our special instructions. Taking therefore this result of rubric b, about 40%, as our standard for comparison with the influence of definitely directed expectation, we may now proceed to show the quantitative extent of this influence, which itself is of a twofold nature. According to the direction of expectation, its influence upon the forward-backward confusion is either of a restricting or of an enhancing nature.

The former, the restricting influence, is plainly shown under rubric g, where expectation is limited to the same front or back half in which the stimulus is actually given; because in these cases the smallest errors in the displacement of sound occur, averaging a total of 32.7% for all stimuli and a reduced total of 23.8% for the intermediate stimuli exclusive of 0, ± 90, and 180. In other words, when expectation is directed to the same front or back half in which the stimulus is to be given, the usual error of sound-displacement due to forward-backward confusion is reduced more than one third. The contrary results occur under rubric f, where expectation is directed to the front or back half opposite to that in which the stimulus is actually given. Under these conditions, the forward-backward confusion is enhanced; the standard error of 40% is increased to 70% if all positions are included, and to 57.6% if the corner-positions are excluded. Considering only the latter figure, we may say that expectation increases the usual error by almost one half. Comparing the absolute differences between the standard error of displacement, 40%, and the modifications due to expectation, we find that the enhancement is practically as great in the one case as the restriction in the other case. These results, then, solve the particular problem for which our experiments had been originally planned. We may summarize our conclusion by saying that the original error of sound-displacement due to the usual forward-backward confusion is either increased or decreased by almost one half according as expectation is directed to the opposite or to the same front or back half from which the stimulus is to be given.

In addition to the results and conclusions thus far reached our data furnish a number of other results which we may now discuss. A study of the mean variations in Table I will show consistent differences with regard to certain stimulus-positions. The mean variations for 0 and 180 are greatest, then follow ± 30, and last of all ± 120 and ± 90. This result indicates clearly that the mean variations do not depend upon the inaccuracies of the individual observers, but rather upon the difficulties of localizing sound from certain directions. The question of individual differences must therefore be approached from different angles.

The attitude of the individual observers towards the various kinds of instructions, and their individual degrees of accuracy in localizing sound, are indicated in Table III. In the case of the instruction "unlimited," the last three of the eight women observers (rows 6-8) and the first nine (rows 10-18) of the twelve men are very consistent in voluntarily limiting their expectation to definite points in the circle, as the first three rubrics show; while the other observers seem to waver between this method and no (or at most an indefinite) expectation. With limited expectation the grouping is practically the same, except that one of the women (row 6) joins the wavering group and one of the men (row 19) joins the consistent group. This

difference in attitude seems to have, however, no appreciable influence upon the individual's accuracy of localization, according to the size of the average errors listed under the last four rubrics. Similarly, if we compare all observations under limited indefinite expectation with those under limited definite expectation without regard to the individuals, as is done in Table IV, rubrics k and l, we find, for the first group of experiments, at least, a very small and irregular difference in favor of definitely limited expectation. The same fact holds true with the second group of experiments, except that here the difference is somewhat larger, because most of the cases involved are from observers whose total average errors for the quadrants are considerably larger than the average for all 20 observers. It seems therefore indifferent, for the accuracy of localization, whether one expects the sound from a well-defined point or from a wider and less defined region of the periphery.

TABLE III

Rows		Instruction:	Unlimited or None			Limited Exp'n.		Total Average Errors			
		Expectation:	None	Indef.	Vol. Lim.	Indef.	Defin.	Unlim and H.	Quadrants	Combined	%
		Observers:	1st Group Expts			Both Groups		Both Groups of Expts.			
1	Women	R.R.	6	3	3	24	32	27.7	29.9	28.1	30.6
2		M.E.J.	9	0	3	29	27	32.3	14.4	18.2	30.8
3		P.O.	0	4	8	2	54	43.9	21.2	38.5	42.0
4		S.L.	8	0	4	17	39	43.3	37.5	41.9	45.7
5		H.M.	7	0	5	19	37	45.8	44.4	45.4	49.5
6		J.O.	0	1	11	10	46	25.8	30.0	26.6	29.1
7		J.N.	1	0	11	0	56	30.6	27.5	30.0	32.5
8		D.S.	1	1	10	2	54	45.2	39.4	43.9	47.7
9		Wom. Tot.	32	9	55	103	345	36.8	31.8	35.4	38.5
10	Men	H.M.P.	1	2	9	3	53	49.0	11.2	40.1	43.7
11		H.B.	0	0	12	0	56	43.3	33.1	40.9	44.9
12		L.M.	0	0	12	0	56	42.3	41.2	42.0	45.8
13		H.D.W.	0	0	12	0	56	44.2	39.4	43.1	47.0
14		T.N.H.	0	0	12	1	55	45.8	37.0	43.7	47.6
15		W.E.H.	0	0	12	0	56	46.1	36.2	43.8	47.7
16		E.R.P.	0	0	12	0	56	48.3	33.1	44.7	48.7
17		J.E.M.	0	0	12	1	55	47.3	42.0	46.0	50.2
18		J.M.	1	0	11	0	56	55.5	28.1	49.1	53.5
19		T.A.M.	7	0	5	0	56	42.5	26.2	40.7	44.4
20		G.B.	2	9	1	35	21	45.0	47.5	46.0	50.0
21		F.H.	11	1	0	56	0	41.5	37.5	40.6	44.2
22		Men Total	22	12	110	96	576	46.1	34.4	43.4	47.4
23		Grand Tot.	54	21	165	199	921	42.4	33.3	40.2	43.8

The question of sex-difference in our experiments is complicated by the presence or absence of general scientific and specifically psychological training in our observers. Tables III and IV show a remarkable and consistent difference in accuracy of sound-localization of about 10% in favor of the psychologically untrained eight women observers. This difference cannot be explained on the basis of expectant attitude, because the five women exhibiting the indefinite wavering attitude average 39.7% as against the 45.7% of the three

TABLE IV

Rows	Rubrics	k			l			a		d		e		f		g		i		j		m		n	
	Instr.	Total Average and Errors						Unlim. Exp'n.		Exp. Lim. to Right or Left				Exp. Lim. to Front or Back				Exp. Lim. to Quadrant				Total Average and Errors			
		Indefinite Exp'n.			Definite Exp'n.			General Results		St. opposite		St. same		St. opposite		St. same		St. in diff.		St. in same		8 Women		12 Men	
	Stimulus Position	Rubrics a and d–g						W	M	W	M	W	M	W	M	W	M	W	M	W	M	Rubrics a and d–g			
		No.	Av.	E.	No.	Av.	E.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	Av.	E.	Av.	E.
1	0	7	124	124	53	143	143	122	144	...	...	...	...	109	163	79	131	...	...	...	...	103.3	103.3	146.0	146.0
2	+30	26	102	72	114	107	77	116	122	85	125	119	120	116	132	71	79	114	108	81	94	99.0	69.0	111.4	81.6
3	−30	23	119	89	117	99	69	100	127	91	112	69	125	95	132	50	90	111	136	75	92	85.0	55.0	116.5	86.5
4	+60	15	89	29	125	86	26	94	83	92	79	90	94	104	97	77	72	104	94	72	62	90.6	30.6	83.7	23.7
5	−60	20	70	10	120	89	29	109	80	75	97	77	91	96	92	79	74	not given		66	76	83.8	23.8	85.0	25.0
6	+90	14	90	0	46	83	7	92	82	90	92	79	85	...	...	...	...	...	...	...	...	85.5	4.5	85.3	4.7
7	−90	5	100	10	55	75	15	97	86	90	87	82	93	...	...	...	...	...	...	...	...	90.0	0	89.0	1.0
8	+120	24	95	25	116	96	24	95	89	95	95	99	93	80	84	105	103	106	101	104	101	97.7	22.3	95.0	25.0
9	−120	21	104	16	119	97	23	112	103	95	102	89	93	82	90	104	96	95	107	99	94	96.7	23.3	98.1	21.9
10	+150	19	126	24	121	131	19	139	129	157	123	129	103	127	103	127	125	142	135	140	141	135.4	14.6	122.0	28.0
11	−150	25	132	18	115	127	23	141	138	147	138	124	126	97	102	129	147	141	139	134	131	130.6	19.4	132.4	17.6
12	180	12	103	17	48	151	29	132	164	...	...	...	...	94	114	177	161	...	...	.....		134.6	45.4	146.3	33.7
13		211 Expts.			1149 Expts.																	416 Expts.		624 Expts.	
14	$\frac{\Sigma \text{ Errors}}{\Sigma \text{ St. Pos.}}$	494 / 1080 = 45.7%			484 / 1080 = 44.8%			43.3% / 45.4%		24.8% / 35.5%		32.5% / 42.0%		28.2% / 39.4%		65.0% / 73.6%		40.0% / 41.8%		24.7% / 30.2%		38.5%		46.0%	

men showing the same attitude. But the greater frequency of this wavering among the women indicates that, as a whole, they made more frequent attempts to comply with the instruction "unlimited" than the men, of whom only two carried out this instruction in its intended meaning, while the other ten nearly always expected the sound from definite points on the circle. There was also a much greater variety as to training and age among the women than among the men, and therefore these factors cannot be used as a satisfactory basis for explaining the sex-differences. The smallest difference (about 2%) occurred when expectation was to be "unlimited" and when it was directed to quadrants different from those of the stimulus-position (rubrics a and i); next in size is rubric j (5.5%), then g (8.6%), and finally d, e, and f, which vary from 9.5 to 11.2% in their difference; but always the women's results are better than those of the men. With regard to the stimulus-positions no consistent errors between the two sexes can be found, as rubrics m and n will show. We have no suggestions to offer in explanation of these sex-differences.

Our results reveal furthermore to what extent sound-localization is more accurate in various regions of the circumference. Referring from that point of view to Table I again, especially rows 6 and 14, we see that in general (rubric h) the earward displacement from stimulus-positions in the back half (always omitting $\pm$ 90) is on an average over twice as great as that from the front half. In particular, this difference is least marked under rubrics j and f, then follow in ascending order rubrics e, g, d, a, and lastly i, where it is greatest. The localization on the right side is uniformly somewhat less accurate than on the left side, the total average errors (exclusive of positions 0 and 180) being 34.7% for right against 31.2% for left; and for the various rubrics the following corresponding pairs of figures obtain, arranged again in ascending order: smallest or no difference for f, 50.4 *vs.* 50.6%; then g, 25.1 *vs.* 23%; d, 28.5 *vs.* 26.5%; a, 34 *vs.* 28.4%; and greatest for e, 37.8 *vs.* 30.5%. This result is somewhat surprising, as practically all our observers were right-handed, a fact which would lead us to expect the opposite result. There is no difference as to sex in this respect, as the corresponding total average errors for the women are right 28.2 *vs.* 25.2% left and for the men 32.6 on the right *vs.* 30.9% on the left side. We have at present no explanation to offer for this superiority of the left over the right ear.

We are now ready to discuss the introspective reports on the mental processes of expecting and localizing sound as required from ten of our male observers. These reports are given here in full, partly for the sake of avoiding arbitrary selection, and partly in order to show the quality of introspective work which may be expected and obtained from undergraduates with average and better than average college records. The observers were told to subdivide their report into a pre-stimulation or expectation-period (A), including the conscious facts from the perception of the instruction up to the perception of the stimulus, and a localization-period (B), beginning with the sound perception. The observers either wrote out their reports themselves or dictated them to the recorder. No suggestive questions were asked by the latter; but whenever a word or phrase in the report did not seem clear to him, he demanded further details, which were to be expressed as far as possible in technical terms. The reports are numbered in accordance with the rows 10 to 21 of Table III. The small letters in parenthesis in the first part of each report refer to the cor-

responding items in the subsequent summary outline of the expectation-period, while the bracketed letters in the second part correspond to the items in a similar outline of the localization-period. The abbreviations employed are either self-evident or will be explained in the subsequent outlines.

10. A. Vis. Im. of instrument from where expected (a). Im. is projected into external space (t). Musc.-Kin. Tendency of eyes to turn (l) toward expected im. Kin. Tend. to turn head (f) toward expected noise. With "Unlimited" instruction (o) the position of instrument located by projected image (t).

B. Auditory sensation (a). If it comes from unexpected place, the original vis. im. fades and a new vis. im. (e) appears. Kin. Tend. to straighten up after sound localisation (j). If uncertain about right place, tend. to turn head (h). Sensation is pl. (q) when coming from expected direction, otherwise slightly unpl. (r). Vis. and auditory im. (e) of telephone receiver.

11. A. Conscious of kin. strains in head (f) and esp. in forehead. Kin. tend. to turn eyes (l) toward expectation-point when it was in the right or left front quadrants (n). When exp'n. was to back half (n), a kin. tendency to turn entire head (f). Whenever expected from 180°, a faint aching pressure (q) in middle of forehead and slightly toward right eye. Exp'n. of direction occurred only as kin. tend. of turning toward certain point in scale, which later in the report is translated into verbal ideas of figures [cf. B. (o)—]. All exp'n figures should be modified by the word "about" (t). In two or three cases a slight jar of apparatus gave a cue for exp'n. (s), although instructions for one or two of these were in a different direction. In the other cases the jar did not influence the direction of expectation (t).

B. After perception of sound (a), kin. tend. to nod head in direction of lines (h). Verbal ideas of reporting (o).

13. A. Vis. Im. of circle and points in circle (b). Sometimes kin. pointing in direction of instruction (k). Perhaps also eye-movements (h) in same direction. Visualised instrument at certain point (a). Several times did not expect sound according to instruction (r).

B. Sound had a space-attribute at once, and afterwards pointing, (b) and (m). Visualised a large circle on floor (g), sometimes with Vis. im. of instrument at certain point (e). Sometimes a feeling of surprise (s) slightly unpl. (r), when sound came from another direction.

14. A. Had a visual im. of the part of scale (b) from which sound was to be expected, divided into degrees. Sometimes also verbal im. of certain parts in that scale (e). Kin. strain located in neck (g) and back (j), as if leaning in required direction. When instruction was "unlimited" (o), had a vis. im. of the four points 0, ±90, and 180 (c), and whichever came first into mind was the one from which sound was usually expected.

B. The auditory perception (a) of the sound aroused a vis. im. of the instrument in the particular position (e). Pointed (m) or tended to point (i) in the direction from which the sound was heard, and then translated this into degrees of the scale, (o).

15. A. At the words "Unlimited" (o) a visual image of instrument either at 180 or at 0 (c). Just before signal "Ready" a vis. im. of instrument in front half turning from left to right (d). At the words "Ready" a vis. im. of instrument at some definite spot (a). Sometimes there is a delay in visualising the apparatus, (t).

B. Auditory sensation (a), pleasant when sound came from expected direction (q). Tendency to laugh (n) and nod head in direction from which sound came (h). Verbal ideas of translating vis. im. into terms of scale (o).

17. A. When starting in, my idea was that I could not tell much about it. Was surprised because sound seemed to come from a definite point, clear cut (t), and has been so every time except in a few cases. I had thought also that the point of expectation had nothing to do with it at all (t). At first I had trouble in locating degrees, but later a pretty good vis. im. of instrument (a), of scale with — and + posts, the zero and 180 (b) and (c). When instructed to expect sound from given half (p), I usually imaged the central point of it. When the right or left half was called for, had a peculiar feeling in one of the ears, very dim, resembling a cutaneous pressure (p), a bare consciousness of one ear (t). Sometimes had a musc. tend. in the neck (m) as if leaning the head in the direction of expectation (n). Sometimes my attention wandered to some other point of the scale (s), but almost always returned to the place told.

B. The sound (a) was heard to come from certain points (b). A kin. tend. to point to it (i), and thus the sound locates itself. The placing on the scale was mostly in verbal terms (o).

18. A. Kin. strains all over body (i). Attention directed to some point located in scale (c). Vis. im. of apparatus (a) moving toward this point (d). Vis. im. of a large circle marked in 10° points (b) and (e).

B. Auditory perception of click of instrument (a). Previous vis. im. disappear and are replaced by verbal kin. perceptions and ideas (o) of giving the position of the click. Sometimes attention fluctuated between vis. im. (u) of points 180° apart. Sometimes additional vis. im. of experimenter and recorder and of windows with strip of wall between (f).

19. A. Vis. im. of dial with pointer (a) and (b) with a special reference to red figures (c). The four points 0, ± 90, and 180 were esp. plain (c). Associated app. with a wheel with hub and four spokes at right angles (q). The vis. im. continually went back to 90 (d), but sometimes thinking I would be fooled by experimenter (r), I changed it to other points. If "Unlimited" (o), this figure seemed to revolve without stopping (d) at any place.

B. Localization was accomplished (a) either by actually pointing (m) or imaging myself pointing (k) in the direction of sound. Had a vis. im. of scale at all times (c). Imagined a straight line from sound to myself and up to myself, horizontal lines seemed about ten feet long, (g), as sound seemed to come from far distance and somewhat from below.

20. A. Tried to discover, against my will (t), what the experimenter's regular order of stimuli was; in verbal ideas (e). Expected the stim. in kin. imagery, felt that I could move or point (k) toward

the place without hearing the sound. Had a vis. im. of red letters on scale (c) and verbal idea (e) that the sound would come more frequently from their direction (t). Vis. im. of the two parts of the app. moving (d) set up by the Experimenter's steps (s) and a verbal idea that sound must come from either the one or the other point (r). My kin. position of whole body and chair never felt satisfactory in that it seemed not in the center of the circle (i) and (t). This may be one source of my errors. Had a stronger tend. to locate points in front rather than behind (t), because everything seems to be projected forward in everyday life, a sort of racial habit. Several times I thought that Experimenter would fool me and expected from another than the instructed direction (r).

B. Localization through auditory perception (b) associated with vis. memory im. of instrument and scale (c) and (e). The localization aided by expectation because I concentrated steadily in the expected region (t).

21. A. Imagined myself in center of circle divided up in degrees in vis. terms (b). When Experimenter said which half, I direct my att'n visually to that half as a whole (p), unless some noise by E caused me to pick out special place in vis. im. (s). In these cases I was inclined to lean my head in its direction (m). In second group of experiments I would have a vis. im. of map of quadrant and tend to lean my ear toward it (n) and forget about rest of circle (t). Sometimes a vis. im. moving as of app. moving (d). Twice I expected sound in opposite field from that told, because I had a verbal idea (e) that E. was trying to deceive me (r), and I reasoned out that he would put it somewhere else.

B. With aud. perc. of noise (a) I tried to select on vis. map (c) the nearest place from which I thought the sound came (d), and figure out in verbal ideas (o) the location by adding and subtracting. Sometimes I located sound first by pointing (m), which was sometimes easier, but not always. Had a tend. to bend head (h) in direction, and in some cases, esp. in group 2, I actually bent it toward sound (l). Attention was always concentrated on work (t) and affectively it was mainly indifferent (p).

The introspective items of the expectation-period may be summarized as follows in order to represent the conscious pattern of expectation. The numbers behind each item refer to the different reports in which the particular item has occurred.

A. *Expectation-Period*

Visual Imagery
- (a) of instrument (sometimes in certain position) 10, 13, 15 17, 18;
- (b) of whole scale 13, 14, 17, 18, 19, 21;
- (c) of special parts of scale 14, 15, 17, 19, 20;
- (d) of moving instrument 15, 18, 19, 20, 21;

Verbal Imagery and Associations (e) 14, 18, 20, 21;

Kinaesthetic Sensations of Strain and Actual Movements
- (f) located in head (10), 11;
- (g) located in neck 14;
- (h) located in eyes 13;
- (i) in general or various parts of body 14, 18, 20;

Kinaesthetic Imagery and Tendencies
(k) of pointing 13, 20;
(l) of turning or moving eyes 10, (11);
(m) of moving head (11) 17, 21;
(n) of movements in special directions 10, 11, 17, 21;
Special Sensations, Imagery, and Associations
(o) for "unlimited" instruction 10, 14, 15, 19;
(p) for Halves or Quadrants 17, 21;
(q) in general 19, for special points 11;
Expectation voluntarily changed from instruction
(r) by auto-suggestion 13, 17, 19, 20, 21;
(s) by accidental noises, etc., 11, 20, 21;
Miscellaneous
(t) 10, 11, 14, 15, 17, 20, 21.

A corresponding outline may be made for

B. *Localization-Period*

Perception of Sound
(a) unqualified, by all observers
(b) with direct spatial attribute 13, 17, 20;
Visual Imagery
(c) of scale as a whole 11, 19, 20, 21;
(d) of points on scale 21;
(e) of whole apparatus in certain position 10, 13, 14, 20;
(f) of environment 18;
(g) of imaginary aids 13, 19;
Kinaesthetic Processes
(h) tendency to move or turn head 10, 11, 15, 21;
(i) tendency to point finger 14, 17;
(j) tendency to move whole body 10;
(n) tendency to laugh 15;
(k) images of pointing 19;
(l) actual movements of head 21;
(m) actual movements of fingers 13, 14, 19, 21;
Verbal Ideas of Translating localizations and of Reports
(o) 11, 14, 15, 17, 18, 21;
Affective Processes
(p) indifference 21;
(q) pleasantness 10, 15;
(r) unpleasantness 10, 13;
(s) surprise 13;
Attentive States
(t) steady concentration 20, 21;
(u) fluctuations 18.

These introspective data are not only in full accord with the numerical data from the same observers, but they also cast some light on the processes of expectation and localization of sound and on the tendency of confusing front and back. With regard to the first they show, on the one hand, that the positions 0, ± 90, and 180 are especially emphasized by the visualizing expectation through greater frequency and vividness than any other positions; and, on the other hand, that the prevalence of the positions in the front half seems to be due to the predominating influence of vision upon our whole mental life With regard to the localization of sound, the reports show the impor-

tance of the kinaesthetic and cutaneous processes originating about the head, neck, and eyes. They finally suggest as an explanation of the frequent earward displacement of sound the usual bare ear-consciousness resembling a dim, cutaneous pressure. Nothing is said in these reports about a consciousness of the instruction, because, as must be remembered, the auditory perception of the instruction usually aroused at once and more or less mechanically a definite visual image, sometimes supplemented by kinaesthetic processes, of the region from which the sound was to be expected. With perhaps one exception none of the observers seems to have suspected the least influence of the direction of expectation upon his accuracy of localization, and as for the degree of accuracy itself, all observers seemed to be absolutely at sea at the end of the session. It is, finally, interesting to note that only three observers attributed more or less explicitly a spatial characteristic to sound, while the others make sound-localization an indirect function depending upon the spatial characteristics of the secondary criteria, such as the associated visual and kinaesthetic processes. But we do not wish to emphasize these and other minor details, because it was not our intention, in these brief introspective reports, to secure an analysis of the conscious mechanism of sound-localization.

We may conclude this study with the following summary of our main results and conclusions:

(1) When sound-stimuli are presented in a horizontal plane at the level of the ears, their localization shows a definite confusion of front and back with a tendency to mass the localizations around the aural axis.

(2) This tendency is strongest with sounds coming from the back.

(3) It is either enhanced or diminished to the same extent,—that is, by almost half of the original error due to front-back confusion,—by the proper direction of expectation to or away from the regions of the stimulus-positions.

(4) The tendency toward earward displacement is perhaps due to a general kinaesthetic and cutaneous "ear-consciousness."

(5) Under the negative instruction "unlimited" or "no expectation" the accuracy of sound-localization decreases by 10% of the general average error.

(6) This negative instruction is as a rule interpreted in the positive sense of "expectation *ad libitum*."

(7) Under this instruction sounds are expected in 87½% of the cases from the front half and only in 12½% from the back. In general, sounds are also more often localized in front than in the back.

(8) This predominance of the front half over the back half in expectation and localization of sound seems to be due to the great influence of vision upon the whole of mental life.

(9) The women observers without previous psychological knowledge were about 10% more accurate in localizing sound than the men who had a uniform and moderate amount of preliminary psychological training.

(10) The left ear was found to be uniformly more accurate by about 3% than the right ear.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XXVII. The Influence of Suppressing Articulation on the Favorable Effect of Distributing Repetitions

By Mildred Mould, Lois Treadwell, and M. F. Washburn

In Study Number XXIII from this laboratory, which appeared in the American Journal of Psychology, vol. 24, pp. 580-582, it was shown that a series of hand and arm movements could be learned with fewer repetitions if an interval of one minute was allowed between the repetitions than if the series of movements was performed again and again without interval. Attention was of course distracted to prevent conscious recall of the movements during the one minute interval. This instance of the working of Jost's Law in a learning process where the motor factors involved were not articulatory suggested the possibility that the law might be primarily concerned with motor processes. In the present study we undertook to investigate the value of the one-minute interval in the learning of nonsense syllables where the articulatory or motor factors were strengthened and where they were interfered with or weakened. If Jost's Law is primarily a law of the learning of movements, then we should expect to find the one-minute interval most favorable when the motor processes in learning syllables are most strongly emphasized.

To find out whether this was the case, we adopted a method which enabled us to work with a number of observers almost unprecedented in memory investigations, and as in the case of Study Number XXIII allowed us wholly to eliminate the effects of practice. It was based on the Method of Retained Members. Six series of ten nonsense syllables each were prepared according to the rules laid down by Mueller and Schumann. They were called Series A, B, C, D, E, and F. These series were used by each observer in the same order, that is, A was always the first one used, then series B, and so on to series F. The syllables were printed each on a separate card, and were laid singly and successively before the observer in time to the beats of a metronome set at sixty; two beats were allowed for each exposure and two for the change of cards. The series was in every case presented four times to the observer, who at the end of the fourth presentation recalled as many of the syllables as she could. In the case of two of the six series presented to a given observer, she was instructed to pronounce each syllable silently as she looked at it. In the case of two other series, she was instructed to try to avoid pronouncing the syllables to herself, and to learn them as nearly as possible wholly by vision. In the case of the remaining two she was instructed to articulate silently, as she looked at each syllable, the syllable 'Deb.' These three methods will be referred to as the articulation method, the no-articulation method, and the Deb method. With one of the two series apprehended by each method, an interval of one minute, during which the observer's

attention was distracted, was allowed to elapse between each presentation of a series and the next. With the other one, the interval between presentations was only four seconds, just enough to mark the end of a presentation.

The influence of practice and of the varying difficulty of the series was eliminated in the following way. Twelve different orders of experiment were used, each with a different observer. The first order was: Series A, by the no articulation method, with four second interval; Series B, no articulation, one-minute interval; Series C, articulation method, four seconds interval; Series D, articulation method, one-minute interval; Series E, Deb method, four seconds interval; Series F, Deb method, one-minute interval. The second order was: Series A, no articulation, one-minute interval; Series B, no articulation, four seconds interval; Series C, articulation, one-minute interval; Series D, articulation, four seconds interval; Series E, Deb method, one minute interval; Series F, Deb method, four seconds interval. The third order was: Series A, no articulation, four seconds interval; Series B, no articulation, one-minute interval; Series C, Deb method, four seconds interval; Series D, Deb method, one-minute interval; Series E, articulation, four seconds interval; Series F, articulation, one-minute interval. The fourth order was: Series A, no articulation, one-minute interval; Series B, no articulation, four seconds interval; Series C, Deb method, one-minute interval; Series D, Deb method, four seconds interval; Series E, articulation, one minute interval; Series F, articulation, four seconds interval. The fifth order was: Series A, articulation, four seconds interval; Series B, articulation, one-minute interval; Series C, no articulation, four seconds interval; Series D, no articulation, one-minute interval; Series E, Deb method, four seconds interval; Series F, Deb method, one minute interval. The sixth order was: Series A, articulation, one-minute interval; Series B, articulation, four seconds interval; Series C, no articulation, one-minute interval; Series D, no articulation, four seconds interval; Series E, Deb method, one-minute interval; Series F, Deb method, four seconds interval. The seventh order was: Series A, articulation, four seconds interval; Series B, articulation, one-minute interval; Series C, Deb method, four seconds interval; Series D, Deb method, one-minute interval; Series E, no articulation, four seconds interval; Series F, no articulation, one-minute interval. The eighth order was: Series A, articulation, one-minute interval; Series B, articulation, four seconds interval; Series C, Deb method, one-minute interval; Series D, Deb method, four seconds interval; Series E, no articulation, one minute interval; Series F, no articulation, four seconds interval. The ninth order was: Series A, Deb method, four seconds interval; Series B, Deb method, one-minute interval; Series C, articulation, four seconds interval; Series D, articulation, one-minute interval; Series E, no articulation, four seconds interval; Series F, no articulation, one-minute interval. The tenth order was: Series A, Deb method, one-minute interval; Series B, Deb method, four seconds interval; Series C, articulation, one-minute interval; Series D, articulation, four seconds interval; Series E, no articulation, one-minute interval; Series F, no articulation, four seconds interval. The eleventh order was: Series A, Deb method, four seconds interval; Series B, Deb method, one-minute interval; Series C, no articulation, four seconds interval; Series D, no articulation, one-minute interval; Series E, articulation, four seconds interval; Series F, articulation, one minute interval.

The twelfth order was: Series A, Deb method, one-minute interval; Series B, Deb method, four seconds interval; Series C, no articulation, one-minute interval; Series D, no articulation, four seconds interval; Series E, articulation, one-minute interval; Series F, articulation, four seconds interval.

Thus each of the six methods of apprehension of the syllables was used with each series and in each time position. Results were obtained from twelve sets of twelve observers each, and each observer, of course, had performed a complete experiment involving the presentation of six series. There were thus one hundred and forty-four observers in all; there was no practice error, and no error due to unequal difficulty of the series.

The results were in the form of the number of syllables correctly retained after four presentations of a series. If a syllable reproduced had two letters in common with one that belonged to the series, but the third letter wrong, it was counted as two-thirds right. The total number of syllables retained by the hundred and forty-four observers when each syllable was silently articulated was, with the four seconds interval, 1,001; with the one-minute interval, it was 1,075.4. The one-minute interval gave an advantage of 74.4 syllables, or 7.3 per cent. of the number learned with the four seconds interval. When the syllables were learned with the attempt to suppress articulation, the total number learned was with the four seconds interval 847.9; with the one-minute interval 879.9, showing a gain for the one-minute interval of 32 syllables, or 3.7 per cent. of the number learned with the four seconds interval. When the syllables were accompanied by the silent articulation of the syllable Deb, the total number learned was, with the four seconds interval, 902.3; with the one-minute interval, 920.0. The amount gained with the one minute interval was thus 17.8 syllables, or 1.9 per cent. of the amount learned with the four seconds interval.

The conclusion that unmistakably follows from these experiments is thus that the advantage which an interval of one minute between repetitions gives over an interval of four seconds only is decidedly greater, about twice as great, when the motor processes accompanying learning are emphasized than when the attempt is made voluntarily to suppress them; and more than three times as great with emphasis on the motor processes than with the suppression of them by the substitution of other motor processes. That such voluntary or artificial suppression of the motor processes could not have been complete; that some articulation of the syllables must have successfully resisted it and even accompanied the articulation of 'Deb' is obvious, but there can be little doubt that the association of the motor processes involved in reading the syllables was more effectually secured when they were given free play than when they were interfered with. It will be seen from the figures given above that both the effort to suppress articulation and the silent pronouncing of 'Deb' operated as distractions; the total number of syllables recalled with full articulation was 2,076.4; with effort to suppress articulation it was 1,727.8; with articulation of 'Deb' it was 1,822.3. It also appears that 'Deb' proved less of a distraction than the effort to suppress articulation; perhaps the silent pronouncing of 'Deb' as each syllable was looked at served to steady attention and keep it from wandering.

Within our experimental conditions, we have good evidence that the law of the superiority of distributed repetitions is chiefly concerned with the motor aspect of learning.

XXVIII. Affective Sensitiveness to Colors, Tone Intervals, and Articulate Sounds

By Margaret Babbitt, Marjory Woods, and M. F. Washburn

In Number XXIII of the Minor Studies from this laboratory (this Journal, volume 24, pages 583-585), it was suggested that a numerical value might be obtained which should represent an individual trait hitherto not investigated as such, namely, the degree of affective sensitiveness to certain classes of impressions. By affective sensitiveness was meant the tendency to derive extreme degrees of pleasantness or unpleasantness from a class of stimuli. Evidently affective sensitiveness may be either general, in other words a tendency to have strong affective reactions to all kinds of stimuli, or specialized, that is, a tendency to be strongly stirred affectively by one kind of impression but not by others. The numerical measure which was suggested for affective sensitiveness was as follows: The observer was required to express the pleasantness or unpleasantness of a number of stimuli belonging to a certain class by the use of the numbers 1 to 7; the ratio was then found between the number of '4' or indifference judgments to the number of '1' or very unpleasant plus the number of '7' or very pleasant judgments. This ratio evidently varied inversely with the degree of affective sensitiveness, and is trustworthy so long as neither the numerator nor the denominator is zero.

In the study above referred to, affective sensitiveness to colors and articulate sounds was investigated. In the experiments described in the present study we used three kinds of material; small pieces of the Bradley colored papers (2.9 cm. square) on a white ground; nonsense syllables consisting of an initial vowel and a final consonant; and musical intervals and chords struck on the piano. The object in using three kinds of stimuli was to study further the relations between general and special affective sensibility; to see, for example, whether the existence of special affective sensibility to auditory impressions would be indicated by a higher correlation between sensibility to tones and sensibility to articulate sounds than between either and colors. There were ninety colors (the full Bradley set), ninety syllables, and thirty intervals and chords. The latter comprised all the intervals within the octave, and a number of three toned chords, consonant and dissonant. Since if the musical stimuli had been given one immediately after the other the sequence would have affected their pleasantness, the order of presentation was as follows. First, three colors were laid one after the other on a white ground, and the observer was asked to judge their pleasantness or unpleasantness using the numbers 1 to 7. Then one of the intervals or chords was struck on the piano, and the observer recorded her judgment of its affective value in the same way. Then three syllables were pronounced, with equal force and distinctness, and the observer's judgment of the pleasantness or unpleasantness of each was recorded. The proceeding was continued until judgments had been made of all the colors, tones, and syllables. The same order was used for the individual colors, tones, and syllables with each observer.

There were ninety-seven observers, and the index of affective sensitiveness was calculated for colors, tones, and syllables in the case of each. Since some of the indexes had no finite value owing to the

fact that the observers either made no indifference judgments or no judgments of extreme pleasantness or unpleasantness, so that the numerator or denominator of the ratio was zero, it was not possible to get averages. The observers were arranged in order of affective sensitiveness to each kind of impression, those who made no indifference judgments standing at the head of the list in the order of the number of extreme judgments they made, as the most sensitive; and those who made no extreme judgments standing at the bottom in the inverse order of the number of indifference judgments they made, as the least sensitive. This arrangement is rather rough, but may serve where the number of observers is so large. Pearson rank-difference coefficients of correlation could then be found.

The following results appeared. First, the highest correlation, 36 per cent., existed between affective sensibility to colors and affective sensibility to syllables; that is, where tones were left out. The correlation between syllables and tones was 31 per cent., and that between tones and colors was the lowest, 24 per cent. It thus looks as if there exists a specialized affective sensitiveness to tones, which is responsible for a lower degree of correlation between sensitiveness to tones and that to other kinds of impression. Considering the special character of musical ability this is what we should expect. It was not possible to show, however, that the observers who were particularly sensitive to the affective values of musical intervals and chords were the most musically gifted, nor that those who were specially insensitive had 'no ear,' so far as their own testimony went; no tests were made. The matter was complicated by the fact that the musical observers had often studied harmony, and a chord which the naïve observer might have pronounced very unpleasant was to them 'interesting' by virtue of its significance for resolution. It would also appear from our indexes that there exists a slight degree of special affective sensitiveness to auditory impressions as such, which is responsible for the higher correlation between tones and syllables as compared with that between tones and colors.

Secondly, our results show that affective sensitiveness to tones is greater than that to colors, but only slightly, and that affective sensiveness to articulate sounds is markedly less than that to either tones or colors. While it was not possible to average the indexes of affective sensitiveness, this conclusion may be based on the following figures. The number of indexes below 1 in value, indicating high and fairly high affective sensitiveness, was for tones 64, for colors 60, and for syllables only 29. The number of observers who never made a judgment of indifference was for tones, 10, for syllables 1, for colors, 1. The number of observers who never made a judgment of either extreme pleasantness or extreme unpleasantness was for tones, 0, for colors, 0, and for syllables, 6.

Finally, our results tend to indicate that general indifference is a more marked phenomenon than general affective sensitiveness. The lists of the first thirty observers as regards affective sensitiveness to each of the three classes of stimuli contain nine names that are common to all three lists (33 per cent.), representing those observers whose affective sensitiveness was general, who tended to be strongly affected, pleasantly or unpleasantly, by all the classes of stimuli. The lists of the last thirty observers as regards affective sensitiveness to each of the three kinds of stimuli contain thirteen names which are common to all three lists (43 per cent.), representing those observers who were markedly indifferent to all kinds of stimuli used. If this excess

of general indifference over general sensitiveness should prove to be a universal law, it might well be due to a failure on the part of a certain number of observers to give adequate attention to the stimuli, which would result not only in indifference judgments but in the suppression of whatever special influences might otherwise be exerted by special classes of stimuli.

XXIX. The Influence of Fatigue on Affective Sensitiveness to Colors

By Harriet Robbins, Dorothy Smith, and M. F. Washburn

In Minor Study Number XIV from the Vassar laboratory (this Journal, volume 22, pp. 112-114), there were reported some experiments tending to show that colors are judged to be less agreeable at the end of a long series of such judgments than at the beginning; that is, that the fatigue induced by a long series of judgments on the affective values of colors lowers their affective value. Our object in the present study was to see how fatigue thus brought about would influence the affective sensitiveness of the observer to the colors; that is, not his tendency to find the colors pleasant or unpleasant, but his tendency to make judgments of an extreme rather than a moderate degree of either pleasantness or unpleasantness. Our method was somewhat more exact than that used in the former study.

The ninety colors of the Bradley series, represented by pieces of the colored papers 2.9 cm. square, were divided into four sets, designated as A, B, C, and D. Sets A and C were of twenty-two colors each; sets B and D of twenty-three colors each. The colors were chosen in random order to make the sets, but the pieces of paper were then numbered on the back so that the colors of a set should always be presented in the same order. Each piece of colored paper was laid by itself on a white background before the observer, who was asked to judge its pleasantness or unpleasantness using the numbers 1 to 7 in the usual way. The entire ninety colors (four sets) were presented thus twice over without pause, making 180 judgments of affective value at a sitting. The set that was used first at a sitting was also used last at the same sitting: thus, one order was: A, B, C, D, B, C, D, A; another, B, C. D, A, C, D, A, B. The observer was asked at the close of a sitting whether she felt tired or bored. For the series which had been presented at the beginning of the sitting and again at the end the following numerical values were calculated; the average affective value of the colors on their first presentation and on their second presentation (that is, the numbers obtained by averaging the numerical values assigned to the individual colors of the set), and secondly, the coefficients of affective sensitiveness for the colors on their first and on their second presentation (that is, the number of judgments '4,' or indifferent, divided by the sum of the numbers of '1,' very unpleasant, judgments and '7,' very pleasant judgments.)

There were 82 observers, all young women and undergraduate students. We were rather surprised to find that only 31 per cent. of them declared themselves bored at the end of the long series of 180 judgments. Five of the 82 observers said that they became more interested towards the end than they had been at the beginning.

Of those observers who reported being conscious of boredom, 34.6 per cent. showed higher affective values for the colors at the end of the series than for the same colors at the beginning, and 65 per cent. showed lower affective values. A difference of 30.4 per cent. represented the tendency of affective values to drop as the result of ennui. Of those observers who reported equal interest maintained throughout the experiment, 36 per cent. gave an average affective value higher for the colors on their second presentation, and 56 per cent. an average affective value lower for the colors on the second presentation: a difference of 20 per cent. indicating the observers who in spite of not recognizing boredom introspectively, showed a fall in the average degree of pleasantness assigned to the colors at the end of the series. Of the five observers who said they were more interested in the colors at the end than at the beginning, three showed a rise in the average affective values assigned to the colors and two a fall.

Turning to the coefficients of affective sensibility, we find that among the observers who reported ennui, 23 per cent. showed increased affective sensibility and 53 per cent. decreased affective sensibility, a difference of 20 per cent. in favor of the conclusion that ennui lowers the affective sensibility. Among the observers who reported equal interest throughout, 43 per cent. showed increased affective sensibility and 38 per cent. decreased affective sensibility. All of the five observers who said their interest increased showed increased affective sensibility.

These results indicate that, under the conditions of our experiments, affective sensibility to colors tends to diminish with ennui produced by a long series of judgments on the affective values of colors, and that diminution in affective sensibility is more closely correlated with introspective reports of ennui than is the average affective value of the colors. The percentages of observers who showed a lowering of the affective values of the colors were 65 for the observers who reported ennui, 56 for those who reported equal interest throughout, and 40 for the few who reported increased interest. The percentages of observers showing decreased affective sensibility were 53 for those reporting ennui, 38 for those reporting equal interest, and 0 for those reporting increased interest.

XXX. The Source of Affective Reactions to Articulate Sounds

By Louise N. Garver, Josephine M. Gleason, and M. F. Washburn

So far as we know, the first experimental investigation of the pleasantness and unpleasantness produced by articulate sounds was made in this laboratory, the results being published in this Journal, volume 23, pp. 579-583. The object of this study was to find what consonants and what vowels are most agreeable and most disagreeable when used in nonsense syllables composed of an initial vowel and a final consonant. During the experiments made in the investigation the question naturally suggested itself as to why certain articulate sounds should be agreeable and others disagreeable. The present study is an attempt to answer this question.

The material was again nonsense syllables composed of an initial vowel and a final consonant. The vowel sounds used were a as in ate, a as in father, a as in hat, aw, ee, e as in wet, i as in life, i

as in hit, o as in ope, o as in hot, oo, u as in hut, ow, and oi. The consonants included the hard checks p, t, and k; the soft checks b, d, and g; the breaths f, v, th as in breath, th as in breathe, sh, zh, s, and z. Each syllable was pronounced clearly to the observer, who was asked to record her judgment of its pleasantness or unpleasantness by using the numbers 1 to 7, and after doing so to give from introspection the reasons which made the sound seem pleasant or unpleasant. The total number of syllables used with each observer was 151, all combinations making sense being excluded. The syllables were presented in the same order with each observer, so as to exclude the factor of affective contrast. The total number of observers was sixty-five. The great majority were, as usually in our studies, young women college students, but there were seven persons of greater maturity among the number, and also seven men, too small a number, of course, to permit of any comparison between groups.

Two sources at once suggest themselves for the affective reactions made to meaningless articulate sounds: the associative suggestions which the sounds have in spite of their meaningless character, and the ease or difficulty of the articulatory movements involved in pronouncing them. The first conclusion which we have been able to draw from a study of our results is that extreme judgments of pleasantness or unpleasantness, judgments of 'very pleasant' or 'very unpleasant,' are more apt to be due to associations than are judgments of moderate pleasantness or unpleasantness. The per cent. of 'very unpleasant' judgments due to associations with the nonsense syllables was 24; the per cent. of 'moderately unpleasant' judgments due to the same cause was 12.3; that of 'slightly unpleasant' judgments was 12.6; that of 'slightly pleasant' judgments was 13.4; that of 'moderately pleasant' judgments 16.4; that of 'very pleasant' judgments 29.9. We might conclude that there is little about the character of an articulate sound itself, aside from the associations it suggests, to give rise to extreme affective reactions.

The nature of the associations suggested by the sounds may be briefly summarized. First, there were many associations made directly with words, as when the sound 'ool' suggested 'pool.' Or the word association was indirectly made, as when 'ōg' suggested 'fairy tale,' obviously through 'ogre.' Secondly, a sound often suggested the object making a similar sound: 'ees' suggested mice. Thirdly, a sound suggested that it was a corruption of a word through dialect or some defect of articulation. Fourthly, a sound sometimes suggested a sensation associated with a word meaning: 'ife' felt sharp, 'oothe' felt like a hand stroking the shoulders; both clearly indirect word associations. Finally, there were a number of unaccountable associations with the sounds. 'Oig' suggested a sickly grin, 'ofe' a witch, 'oif' a giant, and so on. Very likely these were cases of 'mediate' verbal associations.

It appears, as a second conclusion, from the figures given above that associations contribute more to the pleasantness than to the unpleasantness of articulate sounds. The aggregate of the percentages of unpleasant judgments due to association was 48.9; that of the percentages of pleasant judgments due to association was 59.7.

A third conclusion is that the pleasantness or unpleasantness derived from ease or difficulty of articulation is more likely to be of a moderate than of an extreme character. 2.8 per cent. of the judgments 'very unpleasant' were due to the unpleasantness and difficulty of articulation; 3.7 per cent. of the 'moderately unpleasant' judgments,

5.5 per cent. of the 'slightly unpleasant' judgments. 4.2 per cent. of the 'slightly pleasant' judgments were assigned to ease and agreeableness of articulatory movements as a cause; 4 per cent. of the 'moderately pleasant' judgments, and 3.2 per cent. of the 'very pleasant' judgments. It will be noted, finally, that the balance here is a little on the side of unpleasantness; the percentage of judgments 'slightly unpleasant' is the largest due to articulatory influences.

The cases where no reason was given for finding a sound pleasant or unpleasant include two classes which we have not attempted to separate: those where the unpleasantness or pleasantness may really have belonged to the sound itself, and those where the observer's introspection was unequal to the task of detecting the reasons for her own reaction. It is a question which goes to the root of aesthetics, whether a sensation can have any affective character that does not trace its cause to welfare. Our likes and dislikes are traditionally explained as due to conditions that favor or hinder our own good, including that of our species. How can finding a color pleasant or unpleasant have survival value? We can connect articulate sounds with welfare through their associations and through the benefit of free movement of the articulatory mechanisms and the harm of impeded movements. The impossibility of distinguishing between the effects of imperfect introspection and those of affective reactions to pure sounds, aside from associative or kinaesthetic elements, makes us unable to draw any conclusions with regard to the occurrence of such reactions.

Certain observers reacted separately to the consonant and to the vowel in a syllable; that is, they declared that they liked or disliked both, or liked one and disliked the other. In this case the vowel sounds were nearly always declared to be simply agreeable or disagreeable, without further qualification. The consonants, on the other hand, often had some more definitely qualifying adjective applied to them. Occasionally these adjectives referred to an association suggested by the consonant itself, as 'lisping' for th. In other instances the adjectives were descriptive of the consonant sound itself. The checks, especially p. b, and d, were objected to as cutting short the vowel sound too abruptly, as being sudden. On the other hand l, m, and th as in breathe were especially commended for the opposite character of being 'continuous,' 'letting the sound out.' Does the dislike of an abrupt close to the vowel sound rest upon the general association of abrupt movements with violence and constraint? The term 'hard' or 'harsh' was commonly applied to the guttural checks, which are the most unpopular consonants. It is difficult to decide whether this refers more to the abrupt character of these modifications of sound, that is, depends upon a suggestion essentially kinaesthetic, of checked or impeded movement, or whether a real auditory harshness is felt. Certain breaths are criticized as hard to finish; f and v, for example: this is a kinaesthetic unpleasantness. Sh, th, and zh were often condemned as 'sloppy.' This is probably a sound association, a suggestion of the noises made by splashing liquid. That the vowel sounds should have no qualifying adjectives except 'pleasant' and 'unpleasant' is easily understood. There is much more of the purely auditory character and less of the kinaesthetic about a vowel than about a consonant. The long vowels are generally preferred to the short ones (although short e is a very popular vowel) because they are more musical; the tone quality has more time to be appreciated. Musical changes have an affective value in themselves that

it is difficult to trace to any associative source. Articulatory movements, on the other hand, have the same reasons for being pleasant and unpleasant as any other bodily movements; they are agreeable when they are free and unconstrained, disagreeable when they are the reverse. The indications from our experiments are that the only elements in articulate sounds that do not derive their pleasant or unpleasant character from association, either with meanings or with kinaesthesis, are the vowel sounds.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

COMMUNICATED BY E. B. TITCHENER and H. P. WELD

XXIII. ON INTENSIVE AND QUALITATIVE JUDGMENTS OF LIGHT SENSATIONS

By E. J. GATES

Since Hering showed the qualitative character of the black-white series, there has been much discussion regarding the intensive attribute of sensations of light. We sought, in the present study, to find out whether an observer is able to compare two such sensations both qualitatively and quantitatively; whether, that is, it is possible to determine both an intensive and a qualitative limen with the same light stimuli; and, if so, whether the two limens coincide.

Apparatus. We used the Whipple apparatus for discrimination of brightness.[1] The lamp was a 10 watt, 110 volt, frosted Mazda; observations were taken only during hours when the university current is not subject to rapid fluctuation. The reflectors were covered evenly with a blue paper (Milton Bradley blue tint no. 2). The observer sat with his head in a head-rest, and with his eyes distant 50 cm. from the windows of the apparatus. Exposure was made by means of a shutter; the regular exposure was 5 sec.; but if judgment was passed within that limit of time, the shutter was immediately closed. The work was done in a dark room, with adaptation to dark.

Method. The method of limits, as set forth in Titchener's Manual, was followed for the determination of a lower *DL*. The normal stimulus was given by the setting of the pointer at 0° of the scale (high but not maximal illumination); the unit of change for the stimulus of comparison was 2° of the scale. The experiment was so planned that the ascending and descending series, the right and left positions of the normal stimulus, and the instructions for intensive and qualitative judgments, were evenly distributed. In all, 64 series were taken from each observer. Preliminary practice was given, under definite instruction, in order that a standard of judgment might be attained.

Observers and Instruction. The observers were Dr. E. G. Boring (B) and Mr. G. J. Rich (R). The former was highly practised, and had a full theoretical knowledge of the question at issue; the latter was relatively unpractised, and knew no more of the problem than he had learned in a course of lectures on systematic psychology. At the beginning of every series, the observers were told that it would

[1] G. M. Whipple, Manual of Mental and Physical Tests, 1910, 163; i., 1914, 197.

be an ascending or a descending series, and were instructed to judge in terms of intensity (by 'greater,' 'equal,' 'less') or in terms of quality (by 'lighter,' 'same,' 'darker'). No introspective reports were required during the experiment; but at its close the observers were asked to distinguish, as accurately as they could, between the intensive and qualitative judgments and attitudes.

Results. The numerical results were as follows; the units are degrees upon the scale of the apparatus:

	Observer B	Observer R
Qualitative *DL*	25.1±4.1	27.6±2.7
Intensive *DL*	22.2±4.6	24.6±3.1
Qual. *DL* ascending	23.5±3.8	26.4±2.3
Qual. *DL* descending	28.7±4.2	28.9±2.8
Int. *DL* ascending	19.4±4.0	24.1±3.9
Int. *DL* descending	25.1±3.2	25.1±2.3
Qual. space error	—3.4	—1.6
Int. space error	—3.1	—1.6

The probabilities that the difference between the qualitative and the intensive *DL* (2.9, 3.0) is significant are, in the case of B, nearly 100:1 and, in the case of R, enormously greater.

Discussion. It is clear that the two observers have approximately the same *DL;* and it is clear, from the trend of the figures, that they adopted similar attitudes to the stimuli. The reports appear to show (1) that the instruction for quality brought out ordinarily pure qualitative judgments, but (2) that the instruction for intensity brought out judgments of insistence or *Eindringlichkeit* and not of intensity proper. Observer B writes:

"In judging 'intensity' I tried to give myself up to the quantitative aspect of the experience, to take it with regard to the 'total force of the impression.' The judgment was generally in terms of relevant kinaesthesis; the one which came nearer to 'knocking me down,' as it were, was taken as the more intense. Quality was judged as 'degree of blackness.' I do not know whether I could have done my series in terms of white, *i. e.,* taken the ascending series as meaning 'more white' and the descending as 'less white,' but it does not seem to me now that I could. The judgment 'lighter' was particularly hard to give, and in those descending series where I did give it, I actually shifted the basis of judgment. . . . I was early aware that the qualitative limen seemed to be greater than the intensive, and this fact may have influenced my judgments. I was generally aware of both changes in a given series; and in the descending qualitative series would not expect a change until the intensive change had appeared; contrariwise in the ascending intensive series. I was less certain of the qualitative judgments than of the intensive. This was due, I believe, to the fact that the equality of the intensive series meant more nearly a total identity than did the 'same' of the qualitative series. It is always easier to judge of identity of two impressions in respect to all their attributes (cf. color-mixing) than

to judge of identity of a single pair of attributes when the others differ."

B, then, judged 'intensity' by total impression or insistence, and naturally found these judgments easier than the qualitative.[2] R writes, less analytically:

"I had considerable difficulty in getting criteria of difference between quality and intensity. I presently came to take intensity, not as intensity of visual sensation, but as intensity of this whitish sensation; however, it was always more or less of a qualitative thing. As the variable slit was changed from sameness or equality, the first difference I noticed was what I reported as a difference in intensity. It came in very gradually, but was not difficult to judge. A little further along there was another change, and this I reported as difference in quality; towards the end of the experiment I was surer of it."

In the light of B's report, it seems permissible to say that R, too, judged 'intensity' by total impression or insistence.

Referring now to the numerical results, we see that the intensive limens are in fact significantly smaller than the qualitative limens which correspond with them; though the subjective assurance of the observers is not reflected objectively in the values of the *MV*. It is noteworthy, however, that the two series which are mentioned by B as involving the dual criterion (the descending qualitative and the ascending intensive) give the extreme liminal values: the descending qualitative goes lower than its pair towards black, and the ascending intensive goes higher towards white.

Remarks on Method. We made various attempts (gelatines, tissues) to eliminate the yellowish quality of the stimuli, and finally had recourse to the blue paper spread on the reflectors. In the preliminary series this paper was judged to be effective. In the experiment itself, R did not notice color; but B writes: "The stimuli were noticeably yellow and seemed more so at some times than at others. I constantly and consciously abstracted from the color, which was bothersome, however, only in the intensity series." If the experiment is repeated, the yellow should be more adequately compensated.

The exposure-time of 5 sec. is probably too long. The observers knew their method, and realized that they were to give immediate judgments. In cases of difficulty, however, they utilized the full 5 sec. B writes: "I was bothered at times by the fear that I was considering an extensive difference in the qualitative series; for in cases of long exposure the 'blackness' seemed occasionally to appear in patches. I am not sure of this, however." Care was taken, of course, that there should be no interference by after-images; but an exposure-time of 3 sec. would probably have simplified the experiment. We chose the longer time with the idea that the judgments would thereby gain a surer and more definite basis.

[2] Cf. the ease of judgment reported by L. G. Meads, this JOURNAL, xxvi., 1915, 151. As B observed in this study, and may therefore be thought to have been influenced by suggestion in the present work, it is well to state that our series were completed before Meads' were begun.

Conclusion. We conclude, so far as this study goes, that 'intensive' judgments of light sensations are likely to be founded upon insistence or 'power to catch the attention' rather than upon intensity itself. We think it possible that some of the extant determinations of the *DL* for 'brightness' are measures of this insistence rather than of intensity or quality. Whether it is possible by more rigorous instruction, inducing a conscious abstraction from other attributes, to secure a true intensive *DL* of light sensation remains an open question.

BOOK REVIEWS

Il metodo delgi equivalenti. Contributo allo studio dei processi di confronto. By AGOSTINO GEMELLI. Firenze, 1914. Libreria Editrice Fiorentina. pp. 344.

This monograph is an attempt to work out experimentally, in the field of the comparison of cutaneous distances for different parts of the body, those factors that may influence the method of equivalents. The treatment naturally falls into three parts:—1. an experimental attack upon the factors involved in the method from the purely quantitative side; 2. a statistical attack upon the influence of the factor of the use of visual imagery in making the judgments; and 3. an attempt at an introspective analysis from the qualitative side of the comparison judgment under his experimental conditions. The work was prepared under the guidance of Professors Kiesow and Külpe and clearly shows the influence of both men.

Gemelli invented a new aesthesiometer for his experiments, which not only has the advantage of being exceedingly handy and usable, but also,—and this is of much greater importance,—which enables one to control the intensity of the stimulation as well as the distance between the points. In his experiments, Gemelli employed that technique of the method of equivalents which has become known as procedure G_1. The following parts of the body were stimulated in successive fashion:

1. forehead—lower arm;
2. lower arm—chest;
3. lower arm—back;
4. forehead—chest.

The standard distances varied from threshold values to 12 cm. The ratios of the subjective equivalents to the objective values of the standards are found to be very similar to those obtained by Miss Washburn. These ratios set in with very high values and decrease as the extent of the standard increases. This decrease is found to be constant up to standards of about 8 cm. and then the values of the ratios increase again. The value of this ratio also depends on the sensitivity of those parts of the body that are stimulated, and it is found that the ratio becomes larger the greater the difference in sensitivity of the two points stimulated. This Gemelli believes is an index that the ratio increases along with the difficulties that present themselves to the subject in comparing the two distances. Another factor studied is the effect of the intensity of the stimulation or the amount of pressure exerted by the stimulus. It is found that as the pressure is increased the ratio more nearly approaches unity. Hence Gemelli studies experimentally various factors that may influence the results of this method in the tactual field. He makes no effort to study those formal considerations which constitute the basis of the method, nor does he attempt any consideration of the formal relations between this and the other psychophysical methods.

Gemelli now attempts to determine statistically the effect of the

presence or absence of visual images in a comparison judgment under these conditions. This is studied by determining the ratios for sensations on the forehead and on the arm held in different positions. In one group of experiments the arm is held along the body, while in the other it is abducted as far as possible. It is found that cutaneous distances on the arm are underestimated when that member is extended, as compared with judgments when the arm is held in the normal position. Thus the results show very clearly that distances on any part of the body are underestimated when that part is removed as far as possible from the medium line. These differences Gemelli believes to be due to the presence of visual images when the part is abducted. This contention he attempts to prove by experimentation upon two congenitally blind subjects, who cannot, in the nature of things, make use of visual imagery. For these blind subjects, no such differences in underestimation are to be noted with the arm in the two positions.

In the third part of the monograph, Gemelli attempts an introspective analysis of the comparison judgment under his experimental conditions. For this purpose he employs the method of systematic experimental introspection as advanced by Külpe. Such a comparison judgment is considered to be a thought process and hence is capable of analysis by this experimental method. The single process of comparison reduces to the following steps:

1. Preparation of the subject to the process of comparison;
2. Apperception of the standard stimulus;
3. Pause;
4. Apperception of the comparison stimulus;
5. Formation of the judgment;
6. Expressing of the judgment.

Each of these steps is then analysed; and in regard to the first, the preparation of the subject to the comparison process, the matters of *Aufgabe* and *Einstellung* are discussed. The factors that form the basis of the judgment are found to be of two general sorts. 1. Mediating procedures, which make use of either, a. visual imagery; b. kinaesthetic imagery; or c. the use of categories. This use of categories is obviously the passing of an absolute judgment upon each of the cutaneous distances. Besides these we have 2. immediate procedures which include, a. successive observation of the two distances; or b. relative observations of the two distances. The immediate procedures are utilized by the subjects when the differences between the two distances are very great and hence the judgment is comparatively easy. When the differences between the two distances that are to be compared are very small, the mediating procedures are brought into play.

This monograph shows some very careful work on the part of the experimenter and is of great value for two reasons. In the first place, it has renewed the interest in the method of equivalents. This method, in its present form, does not give a determination of the measure of sensitivity of the subject, but on the other hand, from it we perhaps get a better understanding of the meaning of the point of subjective equality than from any of the other psychophysical methods. Unfortunately Gemelli does not attempt to give a formal analysis of the method, but his analysis of the experimental factors is of the greatest value. In the second place, the work is of great value because Gemelli has attempted an analysis of the comparison consciousness from the qualitative side. All of the psychophysical methods stand sorely in need of such an analysis. Whether the reader agrees or dis-

agrees with the analysis made by Gemelli, does not affect the fact that such an attempt is a step decidedly in the right direction. It would appear to the reviewer that Gemelli has made a more complete analysis of the particular consciousness under observation than has yet been done in any of the Würzburg studies. And this analysis has been made from a purely psychological attitude toward the problem and without any appeal to philosophy. Unfortunately, it seems to the reviewer, that Gemelli does not publish sufficient introspections to confirm his analysis.

Clark University. SAMUEL W. FERNBERGER.

The Influence of Distractions on the Formation of Judgments in Lifted Weight Experiments. By DAVID MITCHELL, Ph. D. The Psychological Monographs, Vol. XVII, No. 3 (Whole No. 74), 1914. pp. 58.

On the basis of introspection most psychologists have divided attention into at least two sorts—voluntary and involuntary. It is the purpose of this investigation to induce two states of mind which may be characterized by these terms and to study the influence of each upon the formation of judgments. The experiments were carried on in the field of lifted weights. The space errors were eliminated and the weights were presented in the first time order. Two types of sound distractions were employed: 1. a continuous sound made by an electric buzzer to which the subject was instructed not to attend, and 2. the counting of separate clicks—from one to six in number—simultaneously with the lifting of the comparison stimulus. "In the first group the distraction was produced by a sensory stimulation without a concomitant mental activity otherwise induced. In the second group, the work of counting the clicks was a prominent feature of the mental process involved" (p. 33). That Mitchell succeeded in inducing the desired states of mind by means of these distractions is attested by the reports of the observers.

The apparatus employed for the production of the distractions was rather complicated but exceedingly clever and efficient. It was necessary to have the distracting sound exactly, or almost exactly, concomitant with the lifting of the proper weight. This the experimenter obviously could not do. So Mitchell devised a means by which the actual lifting of the weight mechanically actuated the distracting stimulus. Hence the differences in time between the lifting of the weight and the starting of the distraction were exceedingly small.

The experiments were so arranged that normal series without distractions were mingled with the distraction series of both types and so were taken under the same objective and subjective conditions. Four subjects were employed in this investigation and in all over 75,000 individual judgments were recorded; a number amply sufficient to give authority to the results. The data was collected in accordance with and subjected to the calculations of the method of constant stimuli as developed by Urban.

The results show that, contrary to the traditional view, a distraction such as those employed in this experiment tends to produce greater precision of judgment; or in other words, in the presence of a distraction the judgments of the subject are more consistent. This is shown by the greater size of the value of h of the Φ (γ) hypothesis for the series in which the distractions were present. Also this presence of distractions increases the sensitivity of the subject. The

interval of uncertainty is much smaller for the series in which the distractions were present than it is where there were no distractions. This influence is seen to be present upon both of the thresholds in the directions of increase and decrease, *i. e.,* both thresholds tend to more nearly approach one another. The influence is greater, however, on the threshold in the direction of increase. These results are in accord with those from the Cornell Laboratory of a few years ago. As a result of this greater influence upon the upper threshold, the point of subjective equality is shifted and assumes a slightly lower value for the series with distractions. Hence the conclusion is drawn that the presence of a distraction causes an overestimation of the weight. This is in contradiction with the traditional view which holds, to express it in positive terms, that an increase of attention tends to increase the intensity of the sensation. This shift of the point of subjective equality is relatively small however.

Perhaps Mitchell's most striking results are those obtained when we compare the different final values furnished by the distraction series, where the distraction was a continuous sound to which the subject did not attend actively; and the other, where the distraction was the active counting of discrete clicks along with the lifting of the comparison stimulus. It is found that the coefficient of precision, the intervals of uncertainty, and the points of subjective equality for these two series are, on the average, almost identical. Hence we may conclude that both types of attention or distraction had approximately the same effect upon the formation of the judgments. It is difficult to believe that two mental states that are introspectively so apparently different should have such similar effects; but, nevertheless, the results of this investigation show that this is the case. Two alternative hypotheses are advanced in explanation of this difficulty. 1. It is suggested that our introspection may not be one of attention itself, but rather of the conditions of attention, and that the processes of attention may be the same whether voluntarily or involuntarily initiated. 2. It may be that introspection gives us the subjective aspect of two very different sorts of process but that these may be of such a nature that the differences succeed in counteracting one another in such a way as to neutralize themselves and so bring about identical results.

This paper is exceedingly significant inasmuch as it is another successful attempt to extend the application of the psychophysical methods beyond the domain of sense-perception. The paper also opens up a very interesting group of problems that must be solved by future research. Furthermore, it indicates most clearly that an introspective analysis, even though complete, is not the final goal of psychology. Because we must then answer the question as to what is the effect of this combination of structural factors. Mitchell gives his numerical results with great fullness so that his study may be more easily comparable with the results of future investigations. He also includes a number of charts of the curves of the psychometric functions of the different judgments under these different conditions, so that the reader has a graphic presentation of these relationships that is exceedingly helpful.

Clark University. SAMUEL W. FERNBERGER.

The Psychology of Learning: an Experimental Investigation of the Economy and Technique of Memory. By E. MEUMANN, Professor of Philosophy at Hamburg. Translated by JOHN WALLACE BAIRD, Clark University. New York, D. Appleton and Co., 1913, x + 393 pp.

This translation of Meumann's Oekonomie und Technik des Lernens is based on the third edition of the German work, which appeared in 1912. The present text approximates a complete exposition of the psychology of memory and its application to practical life and education.

In the preface to the American edition Meumann calls attention to the close community of interest and effort between this country and Germany in the fields of experimental psychology and experimental pedagogy. "It may indeed be said that these sciences have been created by the two nations. . . . In both countries there is a deep-rooted conviction that the most important problems of the science of education can be solved only by an appeal to experimental psychology, and by an application of the methods of psychology to the problems of pedagogy."

The scope of the book may be seen from an enumeration of its contents, although only a few of the more general features can be mentioned at any length. The first three chapters, including the introduction, are devoted to a preliminary discussion of learning and especially to a general analysis of the functions of memory. The relations between observation (perception) and memory have been worked out with great care in Chapters III and IV, in the former of which Meumann gives a very complete analysis of the process of observation and in the latter a resumé of experimental investigations of this function, their methods and results, especially as applied to children. The remaining chapters, comprising more than half of the entire volume, deal with Associative Learning: memory types; economical learning; conditions and technique of mechanical learning; stages of learning; retention and forgetting; the education of memory in the schools; etc.

The author's plan in connection with all these topics is first to make a minute and careful psychological analysis of the functions concerned, second to give a resumé of experimental results, and, finally, to apply the evidence thus obtained to the field of pedagogy. The thoroughness with which this purpose is carried out is admirable, and the book is full of helpful practical suggestions. The evaluation of experimental results is especially good, though one might wish in this connection that the author had drawn more freely from American sources at this point. It may be remarked in passing that of the 136 titles given in the bibliography at the end of the book only 24 are of English and American origin.

Besides the bibliography the book contains three appendixes entitled:

I. The construction of series of non-sense syllables.
II. The meaning of economy of time and energy in learning.
III. Addenda from the most recent literature.

In the introductory chapter, Meumann seeks to make clear the meaning of "technique" and "economy" as applied to the learning process. In order to develop such a technique and promote economy of time and effort, it is necessary to consider not only the general conditions of learning but also more special conditions, such as those which depend on the specific purpose of the learning act and those

dependent on the *will* of the learner. It is worthy of note that a great deal of stress is made at this point and throughout the work on the importance of the will, though no attempt is made to analyze or describe this important factor. "The success of every mental activity, including the act of observing, depends not only upon the accomplishment of the component activities but chiefly upon the co-operation of the will. Will determines how much energy and persistence shall be devoted to the act of observing . . . ." (p. 64.) And again, "We spur and impel the will from within whenever during the course of a long act of learning we detect that the attention is flagging . . . ." (p. 283.) And yet again, "We profit from continued practise only in proportion as we incite the will to progress and arouse an intention to improve." (p. 360.)

In Chapter I, on the Modern Psychology of Memory, we come upon the author's view of general training which is the underlying doctrine of the book. Whenever training in memorizing takes place a two-fold result occurs: (1) the particular material or content is imprinted on consciousness and is thereafter capable of reproduction; (2) the "memory itself, i. e., our general retentive and reproductive capacity, is strengthened." (p. 6.) It is true that Meumann admits later that there is no such thing as "general memory," but the term conveniently expresses the fact that in every act of memory there is a formal training consisting in the increased capacity to acquire practice-dispositions and of setting attention and will to work in the service of memory. Furthermore, the term "general memory" is significant because of the fact that in all cases of the memorizing of special contents improvement takes place in other special memories more or less closely related to the function trained. The well-known results of the experimental work of Ebert and Meumann, on which the latter conclusion is mainly based, are reproduced in detail in a later chapter of the book, but no consideration is given to the divergency of results obtained by Dearborn and other investigators.

The practical outcome of this view is the emphasis on the importance of formal training. "Laboratory investigations of the development of the mental functions of adults show us what an enormous loss is entailed in modern education as a result of its prevailing emphasis upon content as the essential aim in teaching. In view of the fact that the students in our universities must be taught to see correctly and to hear correctly, that they scarcely know what their mechanical memory is capable of accomplishing, that accuracy and fidelity of description are not developed until they come to our laboratories for experiments, that elementary differences of endowment are levelled down so little during the first twenty years of their lives,—in view of all these facts, it appears that our modern system of education fails to fulfill the demands both of science and of practical life. . . . The memory of modern man is wholly neglected, in so far as its formal aspect is concerned; even an improper memory training is better than none at all." (p. 186.)

Another outstanding feature among the more general features of the book is Meumann's stress on the doctrine of types. We have types of attention, of association, of learning, of observation, and of memory and ideation. Individual differences in memory and ideation are fundamental in any account of the learning process and constitute the basis of the psychology of memory and the pedagogy of learning. Thus the typically slow learner is usually of the visual ideational type, though slow learning may also result from slow adaptation of

the learner's attention; the auditory-motor type of memorizer is more rapid, though less sure in learning. All-round development of imagery is the educational ideal. Meumann believes that these types are the result of congenital dispositions but that they are subject, though to a relatively slight degree, to the influence of training and education. Teachers should examine children with a view to discovering their memory types and also their attention types and the essential features of their methods of learning should be brought to the pupils' attention. Formal memory exercises should at first be adapted to the peculiar mental types of the children, but gradually there should be a transition to the development of an all-round training of imagery.

It is fortunate that this work on the learning process by the leading exponent of experimental education in Germany has been made accessible to the wide circle of American readers who are interested in this important field. Professor Baird has succeeded in rendering the work into English of clear and readable character—a task which those who have read the original will agree was no easy one. The detailed analyses, classifications and sub-classifications, added to the somewhat prolix (from an English point of view) style of the author, have not prevented the translator from giving us a work easily understandable by anyone familiar with psychological literature. At the same time the original has been faithfully followed and we now have for the first time in English a resumé of Meumann's views on education.

Yale University. E. H. CAMERON

Grundzüge der Psychotechnik, von HUGO MÜNSTERBERG, Leipzig, 1914. Verlag von Johann Ambrosius Barth. 733 pp. Bibliography.

The Grundzüge der Psychotechnik was first formulated as a series of lectures at the University of Berlin—the first university to offer a course in Applied Psychology, the author tells us. It is dedicated to James McKeen Cattell. The principles laid down in Volume I, published many years ago, and which were worked out without especial changes in other publications, appear now in this—Volume VI—in their true meaning and justification. For according to the belief of the author, an investigation, scientific or otherwise, is of value in proportion as it contributes directly to the common good.

As in the case of other German publications of Münsterberg, this is a systemic treatise, written in textbook style, dogmatic in tone, with much attention to definition, distinction and illustration—but few facts. Indeed, it seems to be the view of the author that facts are relative and negligible—the theory's the thing. And this presentation of the theory is directed to man in his administrative capacity, as the following may indicate "If we are to change the world, to reform and improve men, to teach or to cure them, to make them perform efficient labor or to organize them for common action, then we must treat man as a system of causes which will produce certain effects. We must be able to foresee what will happen and to determine how we can mold the mind." This quotation taken from a corresponding English book by the author, represents fairly, I think, the author's point of view. It is the parent-child relation looked at from the parent's point of view. It is the individual to be manipulated by the administrator, not the individual from his own point of view which is investigated. The practical administrator, then, is the object of Professor Münsterberg's regard—the jurist, the physician, the preacher, the politician, the man of commerce. The book is in-

tended as an introduction to the psychology of his subject, for each of the men named. Suggestion therefore plays a considerable part in the discussions, and the hope of acquiring facility in the art of suggestion for practical ends is the motive to which the book seems to appeal.

But the author hopes that Psychology also will reap some reward. In recent years considerable numbers of men of zeal but little training have undertaken investigations of the individual in his relations to the fields of education, sociology, medicine, ethics, economics, politics, etc. Also, the men actively engaged in these fields look to Psychology for light which they do not find. And furthermore, some Psychologists have gone over into practical fields and have found the problems of real life. While such efforts are praiseworthy, there is a vast waste of effort owing to the vagueness of the fields and the lack of a single point of view; and results of the labor are often useless for want of fundamental principles. These fields of work then, so far as they are concerned with the individual in his mental aspect, need their problems more clearly formulated, legitimate aims set in the foreground, and the fundamental principles so definitely pronounced that order may come of chaos. Under such an Aufgabe, further work might be stimulated and the results become of value, whether this work is done by psychologists, pseudo-psychologists or the men actively engaged in such fields. To supply the deficiency of aim, problems and principles is the purpose of this volume, and it is hoped that Psychology may benefit thereby.

The volume is divided into two parts, of which Part I deals mainly with the implications of the double standpoint in Psychology, its hypotheses and the relations to Applied Psychology. Over against Applied Psychology, we have Theoretical Psychology, which may be subdivided into Causal and Purposive Psychology. The general argument is that Causal Psychology has become too narrow and unproductive; that it has set for itself unnatural and artificial problems; that it has become limited almost to Physiological Psychology. Causal Psychology should have a wider and more useful aim. It should consider, for example, the influences which surround each individual—such as suggestion from without—and influences which are internal—such as autosuggestion, voluntary attention and will. Present day Experimental Psychology, in limiting itself to the content of consciousness, cannot find these influences because they have no content. Nevertheless, the author believes, they are vital forces in our lives.

Part II deals with the special fields of Sociology, Personal and Community Health, Economics, Ethics both social and individual, Education, Art, and finally the relations of Psychology to the Natural Sciences, Historical Sciences, Philology and Philosophy. The rising appreciation of wide individual differences in society, of radical, conservative and socialistic tendencies, of race mixtures, of sex differences, of the young and aged, renders it unnecessary to argue at length as to the need for a science of psychognosis. In the discussion of each of the above fields, the points at which such a psychognosis is needed, are indicated. The advice to the sociologist (or to any one in another field) is usually phrased as follows "Psychology can help here," "This problem cannot be solved without the assistance of Psychology," etc. In similar fashion, the general problems of each field are put under the microscope and are seen to have a psychological aspect and a technical aspect. Neglect of factual material, however, leave the reader somewhat unconvinced as to the exact contribution which

Psychology, as distinguished from Psychognosis can make, and the respects in which the Psychognosis proposed will be superior to that which the Historian or Economist now make. And while the author claims the prime necessity of scientific method for the solution of these problems, it is evidently not the method with which Experimental Psychology is familiar. For the practical man is interested in ends, purposes, personality, Will—in short, in an interpretation of life. And for this an Applied Psychology is needed. Such a Psychology would perhaps be willing to recognize a certain small value in Experimental Psychology of the present day variety—provided Experimental Psychology will lend itself a little more to this view of the necessity of interpreting life—but otherwise, both Theoretical and Applied Psychology can and will neglect Experimental Psychology and proceed with their own true business. Throughout, Professor Münsterberg pays his respects to Experimental Psychology in this vein.

References to experiments already performed by Professor Münsterberg in the field of Applied Psychology, are somewhat vague. "Experience shows," "the analysis shows," "the experiments indicate" are frequently repeated. Experimental psychologists who wish that these experiments had been carried at least to the point of control, and that hypotheses had come from described facts, find therein a meaning of the term "experiment" which it seems highly desirable to avoid. The purpose of an experiment, in the sense in which it appears in this and other writings, seems to be—to convince the experimenter that his previous judgment is correct. But if the conclusions are disputed, there are no records which will show that this conclusion and no other could have been drawn from them. For Münsterberg, an experiment has served its whole purpose when it offers an interpretation to the experimenter. For Applied Psychology, the experiment should yield a suggestion for some situation in practical life. The suggestions may then be ordered, classified, and finally articulated in terms of the theory which preceded the experiment. Following this method, Münsterberg has undoubtedly done his work in masterly fashion. It is a logical, well organized and well balanced presentation. Perhaps the only criticism which Experimental Psychology should offer is that it leaves room for doubt whether or not there are any facts to support the conclusions.

A good working bibliography for each of the applied fields is to be found at the end of the book.

YALE UNIVERSITY. A. H. SUTHERLAND.

The Negro Races: A Sociological Study. By JEROME DOWD. II, New York, The Neale Pub. Co., 1914. 310 pp.

This volume is one of a series on the sociological study of mankind from the standpoint of race. The author is head of the Department of Economics and Sociology in the University of Oklahoma. The first volume, which dealt with the West Africans, was published by the Macmillan Co. in 1907. The present volume is devoted to the Negroes of East and South Africa. A third volume, on the Negroes in America, is in course of preparation.

In the preface to the earlier volume, the author points out the fact that "up to the present time sociologists, in tracing the evolution of society, have constructed theories based upon data selected promiscuously from opposite quarters of the earth and from many different races." This method would suffice if the races had lived

in the same environment and had undergone the same stages of development, which they have not. The first object of the present work is "to establish the fact that each race has its distinctive institutions and special evolution corresponding to the locality in which it lives or has lived." His criticism of the abuse of the "comparative method" by so many writers and his emphasis on the importance of regional studies of human culture are to be commended.

Stress is laid upon the influence of the physical environment, because "that factor is always predominant in the early stage of development, and only diminishes gradually as man strengthens his intellect and adds to his knowledge. This environment first controls man, after which man controls the environment." The last sentence is perhaps a rather too general and antithetical statement of an actual difference between primitive life and civilization with respect to the relations of man and his physical environment.

The Negro territory is divided into a number of geographic areas, such as the cattle zone, the forest zone, the banana zone, the millet zone, the camel zone, etc., each of which is given separate treatment, followed by comparative surveys. In each zone, the several phases of native culture are studied, including the economic, domestic, political, religious, and aesthetic life, customs and ceremonies, and psychological characteristics. The sources from which the author draws his data are mainly authoritative writings by first-hand observers. Secondary sources like Ratzel's "History of Mankind" and Reclus' "The Earth and its Inhabitants" are also used, and rather too much reliance, it would seem, is placed on such popular compilations as "The Living Races of Mankind."

The influence of the physical environment on those complexes of ideas and habits which we call culture is certainly of great importance, and Professor Dowd has rendered a notable service in showing the operation of this influence among the natives of Negroland. He is on more doubtful ground in his views of the relation of environment to bodily traits, more particularly cranial capacity, and of the relation of the size and form of the head to mental characters. A correspondence between size of brain and intelligence, either in groups of people or in individuals (within certain normal limits) is far from proved; indeed the evidence appears to be largely negative. Also such theories as the one, quoted with apparent approval, that "the dolichocephalic type is everywhere more domineering and ambitious, and is generally represented among the higher and ruling classes and is more largely represented in cities" (p. 95) cannot be accepted. In the opinion of the reviewer, the evidence is scarcely sufficient to substantiate a belief in "a correspondence in all the African zones between the size and form of the brain and its activity." Also we are inclined to think too much faith is placed in natural selection as a modifier of brain capacity. Professor Dowd seems to accept the extreme views of Major Woodruff on the influence of geographic conditions on the brain. According to this view, the general trend of increase of brain is away from the tropics; in a tropical country where existence was easy, a large brain could not evolve. Of course this view is connected with the other, that a large brain means high degree of intelligence. The author's position is summed up in his words, "The process of natural selection adjusts the brain capacity to the conditions, and the process is rapid." These views should be revised in the light of such facts and arguments as are found in writings like Boas' "Mind of Primitive Man" and Wissler's article

in the *Journal of Religious Psychology* for July, 1913, on "The Doctrine of Evolution and Anthropology."

The following quotations will further illustrate the author's conclusions. "The Negro races respond to environment in the different zones of Africa just as the Caucasian and Mongolian races form different types in their respective localities. If there is a difference of races in plasticity and responsiveness to external phenomena it is probably in favor of the Negro. There are probably greater diversities of the Negro in Africa than of the Caucasian in Europe and America." "The backwardness of the Negro in Africa is not due directly to lack of mental capacity but to unfavorable environment." (The author must refer here to the initial or potential capacity of the original stock, for he holds that the environment has affected the present hereditary powers of the brain). "If any other race had peopled Africa in early neolithic times, and remained there until now, it would have advanced no higher than the present culture level of Negro." (p. 277.)

In answer to the question whether the Negro could, under a favorable environment, develop to the same degree of culture as the Caucasian and the Mongolian, the reply is that "in view of the known modifiability of races, it seems reasonable to assume that the Negro would be thoroughly capable of evolving a civilization, but such a result would require a long process of natural selection, and freedom from the antagonistic influences of the more fortunate races."

Professor Dowd holds that the most conspicuous mental trait of the Negro is "excessive emotionalism." We will just refer to the experiments by Miss Keller in Chicago which showed negresses to be decidedly more stolid than white women, as regards expression of emotion. We also call attention to these words of Dr. Marett of Oxford: "It may well be that it is not the hereditary temperament of the Negro, so much as the habit, which he shares with other people at the same level of culture, of living and acting in a crowd, that accounts for his apparent excitability." (*Anthropology*, p. 92.) According to Professor Dowd, "races, as individuals, have inherited emotional characteristics, and these must always color the products of their intellects." Also "the instincts of the Negro differ from those of the Caucasian in intensity and direction. Under changed conditions they would, no doubt, be greatly modified, but they would never behave in the same way as those of the Caucasian." (p. 279.) For an excellent criticism of the view that there is an innate equipment for the acquirement of a particular culture, see the above mentioned article by Dr. Wissler.

University of Minnesota. ALBERT N. GILBERTSON

BOOK NOTES

On the cosmic relations. By HENRY HOLT. Boston, Houghton Mifflin Company, 1914. 2 v.

These volumes attempt a comprehensive philosophy. The evolution of the body and the soul and of the universe are first sketched, and the relations between the known and the unknown in the universe are discussed together with the ethical aspects of evolution. This is termed correlated knowledge. The second book, on uncorrelated knowledge, describes telekinesis, molar and molecular, and the same distinctions are applied to telepsychic telekinesis. Then comes auto- and psycho-kinesis. A very long part is devoted to telepsychosis, in which the cosmic soul, the idea, possession in general, heteromatic writing, dramatic possession, Mrs. Piper, the various Hodgson and Newbold reports, Hyslop, the heteromatic script and various other sittings are treated. The third book includes attempts at correlation, the relation between mediumistic and other dreams, the making of a medium, the dream life, pros and cons of spiritism, dreams indicating the survival of death. The book has an admirable index.

Mental medicine and nursing. By ROBERT HOWLAND CHASE. Philadelphia, J. B. Lippincott Company, (c. 1914). 244 p.

This handbook is meant simply as an introduction to the study of mental diseases. The first part treats of the nervous system, neurons and tracts; part two, of apperception, feeling, emotion, sensation, complexes; part three, of insanity, its general symptoms, disturbance of mental elaboration, of the feelings and emotions, will and acts; part four, of mental diseases, auto-intoxication psychoses, psychoses due to organic cerebral affections, and constitutional psychopathic states; while the fifth and last part discusses the patient from the physician's point of view, and nursing, feeding, etc. The book contains fifty-seven cuts.

The individual delinquent; a text-book of diagnosis and prognosis for all concerned in understanding offenders. By WILLIAM HEALY. Boston, Little, Brown & Co., 1915. 830 p.

The author has been for years the director of the Psychopathic Institute of the Juvenile Court in Chicago. The chapter heads are orientations, the individual, mental basis of delinquency, working methods, including psychology, statistics, with methodology, conclusions and theories of treatment. Book two is devoted to cases, types and causative factors, under which are discussed heredity, developmental and physical conditions, peculiarities and ailments, physical anomalies, stimulants and narcotics, environmental factors, professional criminalism, mental imagery, habit, conflicts, abnormal sexualism, epilepsy, mental defect, subnormality, aberration, adolescence, its peculiarities, pathological lying, love of excitement, stealing.

Genetic theory of reality, being the outcome of genetic logic as issuing in the aesthetic theory of reality called pancalism. By JAMES MARK BALDWIN. New York, G. P. Putnam's Sons, 1915. 335 p.

Under genetic interpretations we have first the problem and genetic morphology, then the individual interpretation, the parallel between it and the racial one. Part second treats the development of interpretation and stresses the prelogical character of the early racial interpretation, also its positive character, the religious interpretation, religious reality and negation, logical interpretation, with mediation theories, especially those based on the primitive and transcendant, and the immediacy of synthesis, the demand for an intrinsic one. Part three is on aesthetic immediacy, and deals with the intrinsic synthesis of aesthetics, its interpretation. The conclusions advocates what the author dubs pancalism, which is a theory of reality from which he deduces various corollaries.

Educational psychology; briefer course. By EDWARD L. THORNDIKE. New York, Teachers College, Columbia University, 1914. 442 p.

In this work the author has rather simplified for beginners his three previous volumes, "The Original Nature of Man," "The Psychology of Learning," and "Individual Differences and Their Causes." It is intended as a text for colleges and normal schools. First come the general characteristics of original tendencies, then man's equipment of instincts and capacities, original satisfiers and annoyers, tendencies to move, learning capacity, anatomy and physiology of original tendencies, order and dates of appearance and disappearance of original tendencies, and value and use of them. Part second is the psychology of learning; part three, individual dfferences and their causes.

The psychological researches of James McKeen Cattell; a review by some of his pupils. (Archives of Psychology, No. 30, April, 1914.) New York, Science Press, 1914. 101 p.

The pupils of Professor Cattell, wishing to recognize in some public way the completion of twenty-five years of service to psychology, have attempted to review and digest his work and to develop still further certain lines in which he was a pioneer. The volume, therefore, not only presents in an admirable light the work of a devoted student of psychology but it also bears incidental witness to the high esteem and affection in which Dr. Cattell is held by his pupils.

Scopolamine-morphine anaesthesia. By BERTHA VAN HOOSEN. And *A psychological study of "Twilight Sleep" made by the Giessen method.* By ELISABETH ROSS SHAW. Chicago, House of Manz, (c. 1915). 216 p.

These chapters discuss pharmacology, toxicology and physiological action of scopolamine, its administration for surgical and especially obstetric anaesthesia, typical and atypical cases, report of 5,000 anaesthesias, report of 100 consecutive cases of twilight sleep, mental effects of it.